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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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A JOURNAL

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY, 1813.

ARTICLE I.

Comparative Analyses of the Urine of different Animals. By Mr. VAUQUELIN*.

Few kinds of urine satisfactorily analysed.

THE only kinds of urine, that chemists have hitherto analysed in a satisfactory manner, are those of man, and some of the larger herbivorous animals. Those of the carnivorous animals and *glires* have not yet been examined by any person that I know of.

Comparative chemistry recommended.

If it be acknowledged, however, that comparative anatomy has contributed much to the advancement of physiology, it will also be found, perhaps, that comparative chemistry may be of great advantage to that science.

Urine of birds.

Already has the analysis of the urine of birds afforded results sufficiently interesting and unexpected, to induce chemists to pursue the inquiry in all animals that furnish this fluid, that we may not judge from analogy, which is frequently deceitful. It is with this view, that I have undertaken the analysis of the urine of the royal tiger, the lion, and the beaver; the results of which I here give, till I have time to pursue my inquiry on this subject farther.

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* Ann. de Chim. vol. LXXXII, p. 197.

Urine of the lion and the royal tiger.

Urine of the lion and tiger.

The urine of the lion, and that of the tiger, are perfectly similar in every respect. They have likewise some analogy to that of man, but they differ from it essentially in some important points.

Points in which they differ from that of man.

First difference.—They are alkaline at the very instant they are voided: on the contrary, those of a healthy man are constantly acid.

Ammonia.

It is to the presence of ammonia developed in these urines, that we must ascribe the strong and disagreeable smell they diffuse immediately on issuing from the bladder of these animals.

No uric acid:

Second difference.—They contain no uric acid, either free or combined with an alkali. At least the analysis of these urines four times repeated afforded me no sensible trace of it.

animal food therefore not its source.

The want of uric acid could not but the more attract my attention, as I had considered its formation to be owing chiefly to animal food.

Want of phosphate of lime.

Third difference.—The almost total absence of phosphate of lime.

This might naturally be expected, as this salt is soluble in water only by the help of a superabundance of acid, and the urine in question is alkaline.

Yet this is separated in the kidneys; but probably precipitated by the ammonia.

It appears, however, that the kidneys of these animals separate a certain quantity of this salt from the blood, for I have found slight traces of it in these urines; while the ammonia is formed only in the bladder, where probably it precipitates the phosphate of lime; and this is no doubt the reason why the urine of these animals is almost always turbid when voided.

Their calculi must be phosphate of lime.

Hence, if calculi be ever found in the bladder of these animals, they can be formed only of phosphate of lime, since they contain no other insoluble substance.

Little muriate of soda.

Fourth difference.—The urine of the lion and of the tiger contains but an infinitely small quantity of muriate of soda, while that of man commonly affords much.

Much urea. Other substances.

In these urines we find a large quantity of urea, much disposed to crystallize, and in general but lightly coloured; phosphates of soda and ammonia; sulphate of potash; a mucous matter, and a trace of iron.

These are the points in which the urine of the lion and the royal tiger resembles those of man: but it differs from it, as we have seen, in a sufficient number of points, to consider it as a distinct species.

It is composed of

Component parts.

1, Urea,
2, Animal mucus,
3, Phosphate of soda,
4, ———— ammonia,
5, Muriate of ammonia,
6, A trace of phosphate of lime,
7, Sulphate of potash in large quantity,
8, An atom of muriate of soda.

Urine of the beaver.

Urine of the beaver.

A careful analysis of the urine of the beaver, several times repeated, has taught me, that it has a great similitude with the urine of the common herbivorous animals.

Its contents.

In fact, we find in it carbonate of lime held in solution by a superabundance of carbonic acid; the benzoic and acetic acids; urea, muriate of soda, and sulphate of potash: but no uric acid, or phosphoric salt.

Difference from that of common herbivorous animals.

It differs from them, however, in containing no muriate of ammonia, but a notable quantity of carbonate and acetate of magnesia, which are not found, at least in any great quantity, in the urine of herbivorous animals.

The following is the mode in which I detected the carbonate of magnesia.

Mode in which the carbonate of magnesia was detected.

After having concentrated a certain quantity of the urine by a gentle heat, I decanted the thickened liquor, and washed with distilled water the vessel, on the sides of which the carbonate of lime had settled. I then poured in some sulphuric acid, diluted with water, which produced a frothy effervescence, on account of a mucous matter, which the carbonate of lime carries with it.

Perceiving that the sulphuric acid had acquired a bitter taste in this combination, I dried and calcined the mixture, washed it with a little water, and by evaporation obtained a salt, that possessed all the properties of sulphate of magnesia.

Desirous of knowing by another experiment, whether the urine of the beaver, like that of all other herbivorous animals, contained any muriate of ammonia, I put into a portion of the thickened liquor a bit of caustic potash; and as no smell of ammonia was perceived, even when heat was applied, I concluded, that it contained no muriate of ammonia. But a phenomenon presented itself, that occasioned me some surprise, and made me desirous of discovering its cause. The liquor coagulated into a gelatinous mass. Suspecting that this effect was produced by the precipitation of some earthy substance, I treated the whole of the thickened urine I had with caustic potash; filtered off the liquor to obtain the matter in question; and after it was washed and calcined, combined it with sulphuric acid, diluted with water, and obtained sulphate of magnesia mixed with a little sulphate of lime.

The acetate of magnesia perhaps a product.

Though I have said, that the urine of the beaver contains acetate of magnesia, yet I am not perfectly certain of it. It is possible, that during the evaporation, though effected by a gentle heat, a certain quantity of acetic acid was formed; and that this acted on the carbonate of magnesia, remaining in the liquor in consequence of its being more soluble than the carbonate of lime.

Colouring matter of its food found in the urine.

We commonly find by the colour, smell, and taste of the beaver's urine, and particularly by its property of dying alumed stuffs, the kind of vegetable on which it has fed.

Instance.

In that in question I very evidently distinguished the colouring matter of willow bark, and its keeper afterward confirmed my observation.

Properties of vegetables not always destroyed in the circulation.

There are cases, therefore, in which certain vegetable substances are capable of passing the digestive organs and the circulation, without losing the properties that distinguish them in their natural state.

Presence of iron.

I found also in the urine of the beaver a quantity of iron, that at first astonished me: but on reflecting, that it had been saved in a tin vessel, and that it contained carbonic acid, I believe the greater part of the metal must be ascribed to this vessel.

Component parts of the urine.

The urine of the beaver, then, is composed of

1, Urea,

2, Animal mucus,

3, Benzoate of potash,
4, Carbonate of lime and of magnesia,
5, Acetate of magnesia (questionable),
6, Sulphate of potash,
7, Muriate of potash and of soda,
8, Colouring vegetable matter,
9, A little iron.

II.

Some Account of ZERAH COLBURN, *an American Child, who possesses some very remarkable Powers of solving Questions in Arithmetic by Computation, without Writing, or any visible Contrivance.*

[The present article is copied from a printed paper, which I obtained from Messrs. Johnson and Co., booksellers, in St. Paul's Church-yard. This boy has been publicly exhibited in America and in London, and some time ago subscriptions were solicited for placing him to be educated under the inspection and care of several mathematical gentlemen: but I have been informed, that the plan was relinquished, from some reasons on the part of his father; and he is again to be seen by the public. A subscription is now solicited for publishing a portrait of him on the following terms]

Remarkable powers of computation in a child.

ZERAH COLBURN, a child *just eight years of age*, without any *previous* knowledge of the common rules of arithmetic, or even of the *use and power* of the Arabic numerals, and without having given any particular attention to the subject, possesses (as if by intuition) the singular faculty of solving a great variety of arithmetical questions *by the mere operation of the mind*, and without the usual assistance of any visible symbol or contrivance.

This print will be engraved from a drawing by Mr. Trumbull; and the size of it will be about 12 inches by 10.

The price to subscribers will be One Guinea, to be paid at the time of subscribing: and the plates will be delivered according to the order of subscription.

The following gentlemen (who are well acquainted with the extra-

Remarkable powers of computation in a child.

extraordinary abilities of this child) have kindly undertaken to attend to the progress and execution of the work, and to see to the distribution of the plates, viz. Sir James Mackintosh; Dr. W. H. Wollaston, Sec. R. S.; William Vaughan, Esq.; John Bonnycastle, Esq., Math. Prof.; Francis Wakefield, Esq.; William Allen, Esq., F. R. S. F. L. S; John Guilemard, Esq., F. R. S. F. Amer. S.; Samuel Parker, Esq.; Francis Bailey, Esq.

Subscriptions are received by either of the above gentlemen, or by Messrs. Johnson and Co., No. 72, St. Paul's churchyard: and printed receipts will be given for the same, which must be produced and given up at the time the plates are delivered.

Zerah Colburn is at present to be seen at the Exhibition Rooms, Spring Gardens. Many persons of the first eminence for their knowledge in mathematics, and well known for their philosophical inquiries, have made a point of visiting him: and they have all been struck with astonishment at his extraordinary powers. It is correctly true, as stated of him, that—"He will "not only determine, with the greatest facility and dispatch, the "exact number of *minutes* or *seconds* in any given period of time; "but will also solve any other question of a similar kind. He "will tell the exact *product* arising from the multiplication of "any number, consisting of two, three, or four figures, by any "other number consisting of the like number of figures. Or, "any number, consisting of six or seven places of figures, "being proposed, he will determine, with equal expedition and "ease, *all* the *factors* of which it is composed. This singular "faculty consequently extends not only to the *raising of powers*, "but also to the extraction of the *square* and *cube roots* of the "number proposed; and likewise to the means of determining "whether it be a *prime* number (or a number incapable of divi- "sion by any other number); for which case there does not "exist, at present, any general rule amongst mathematicians." All these, and a variety of other questions connected therewith, are answered by this child with such *promptness* and *accuracy* (and in the midst of his juvenile pursuits) as to astonish every person who has visited him.

At a meeting of his friends, which was held for the purpose of concerting the best method of promoting the views of the father respecting his education, this child undertook, and completely

Remarkable powers of computation in a child.

pletely succeeded in, raising the number 8 *progressively* up to the *sixteenth* power; and in naming the last result, viz. 281,474,976,710,656, he was right in every figure. He was then tried as to other numbers, consisting of one figure; all of which he raised (by actual multiplication and not by memory) as high as the *tenth* power: with so much facility and dispatch, that the person appointed to take down the results was obliged to enjoin him not to be so rapid. With respect to numbers consisting of two figures, he would raise some of them to the *sixth*, *seventh*, and *eighth* power; but not always with equal facility: for the larger the products became, the more difficult he found it to proceed. He was asked the *square root* of 106929, and before the number could be written down, he *immediately* answered 327. He was then required to name the *cube root* of 268,336,125, and with equal facility and promptness he replied 645. Various other questions of a similar nature, respecting the roots and powers of very high numbers, were proposed by several of the gentlemen present, to all of which he answered in a similar manner. One of the party requested him to name the *factors* which produced the number 247483, which he immediately did by mentioning the two numbers 941 and 263; which indeed are the only two numbers that will produce it. Another of them proposed 171395, and he named the following factors as the only ones that would produce it; viz. 5 × 34279, 7 × 24485, 59 × 2905, 83 × 2065, 35 × 4897, 295 × 581, and 413 × 415. He was then asked to give the factors of 36083; but he immediately replied that it had none; which in fact was the case, as 36083 is a prime number*. Other numbers were indiscriminately proposed to him, and he always succeeded in giving the correct factors, except in the case of prime numbers, which he discovered almost as soon as proposed. One of the gentlemen asked him how many *minutes* there were in forty-eight years; and before the question could be written down, he replied 25,228,800; and instantly added, that the number of

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[*] [illegible] had been asserted and maintained by the French mathematicians, that 4,294,967,297 ($= 2^{32} + 1$) was a *prime* number: but the celebrated Euler detected that errour by discovering, that it was equal to 6,700,417 × 641. The same number was proposed to this child, who found out the factors by the mere operation of his mind.

Remarkable powers of computation in a child.

seconds in the same period was 1,513,728,000. Various questions of the like kind were put to him; and to all of them he answered with nearly equal facility and promptitude; so as to astonish every one present, and to excite a desire that so extraordinary a faculty should (if possible) be rendered more extensive and useful.

It was the wish of the gentlemen present to obtain a knowledge of the method by which the child was enabled to answer, with so much facility and correctness, the questions thus put to him: but to all their inquiries upon this subject (and he was closely examined upon this point) he was *unable* to give them any information. He positively declared (and every observation that was made seemed to justify the assertion) that he did not know *how* the answers came into his mind. In the act of multiplying two numbers together, and in the raising of powers, it was evident (not only from the motion of his lips, but also from some singular facts which afterward occurred,) that some *operation* was going forward in his mind; yet that could not (from the readiness with which the answers were furnished) be at all allied to the usual mode of proceeding with such subjects: and moreover, he is entirely ignorant of the common rules of arithmetic, and cannot perform, upon paper, a simple sum in multiplication or division. But, in the extraction of roots and in mentioning the factors of high numbers it does not appear that any operation *can* take place; since he will give the answer *immediately*, or in a very few *seconds*, where it would require, according to the ordinary method of solution, a very difficult and laborous calculation: and moreover, the knowedge of a *prime* number cannot be obtained by any *known* rule.

It may naturally be expected, that these wonderful talents, which are so conspicuous at this early age, will by a suitable education be considerably *improved* and *extended;* and that some *new* light will eventually be thrown upon those subjects, for the elucidation of which his mind appears to be peculiarly formed by nature, since he *enters into the world* with all those powers and faculties, which are not even attainable by the most eminent *at a more advanced period of life.* Every mathematician must be aware of the important advantages, which have sometimes been derived from the most simple and trifling circumstances; the full effect of which has not always been evident at first sight,

sight. To mention one singular instance of this kind. The very simple improvement of expressing the powers and roots of quantities by means of *indices* introduced a new and general *arithmetic of exponents*; and this algorithm of powers led the way to the *invention of logarithms*, by means of which all arithmetical computations are so much facilitated and abridged. Perhaps this child possesses a knowledge of some *more important* properties connected with this subject; and although he is incapable at present of giving any satisfactory account of the state of his mind, or of communicating to others the knowledge which it is so evident he *does* possess, yet there is every reason to believe, that, when his mind is more cultivated and his ideas more expanded, he will be able not only to divulge the mode by which he at present operates, but also point out some *new sources* of information on this interesting subject.

Remarkable powers of computation in a child.

The profits of the present print will be given to the father of this child, in order to enable him to provide a more *suitable* education for his son: and it is hoped that the friends of science, and the public in general, will promote a plan, which promises to be attended with such advantages.

III.

Farther Experiments and Observations on the Action of Poisons on the Animal System. By B. C. Brodie, *Esq. F. R. S. Communicated to the Society for the improvement of Animal Chemistry, and by them to the Royal Society.*

(*Concluded from p.* 268.)

IV. *Experiments with the Muriate of Barytes.*

Barytes poisonous, but less so than its salts.

WHEN barytes is taken into the stomach, or applied to a wound, it is capable of destroying life; but when in its uncombined state its action is very slow. The muriate of barytes, which is much more soluble than the pure earth, is (probably on this account) a much more active poison.

Exp. 5. Mu-

Experiment 5. Ten grains of muriate of barytes rubbed very riate of barytes applied to a wound in a rabbit.

fine, and moistened with two drops of water, were applied to two wounds in the thigh and side of a rabbit. In four minutes he was evidently under the influence of the poison. In a short time he became giddy: then his hind legs were paralysed; and he gradually fell into a state of insensibility, with dilated pupils, and lay in general motionless, but with occasional convulsions. The pulse beat 150 in a minute, but feeble; and it occasionally intermitted. He was apparently dead in twenty minutes from the application of the poison; but on opening the chest, the heart was found still acting, and nearly three minutes elapsed before its action had entirely ceased.

Exp. 6. Solution of muriate of barytes injected into the stomach of a cat.

Experiment 6. An ounce and a half of saturated solution of muriate of barytes were injected into the stomach of a full grown cat, by means of an elastic gum tube. In a few minutes it operated as an emetic. The animal became giddy, afterward insensible, and lay with dilated pupils, in general motionless, but with occasional convulsions. At the end of sixty-five minutes, from the beginning of the experiment, he was apparently dead; but the heart was still felt through the ribs acting one hundred times in a minute. A tube was introduced into the trachea, and the lungs were inflated about thirty-six times in a minute; but the pulse sunk notwithstanding, and at the end of seven minutes the circulation had entirely ceased.

It appears to act chiefly on the brain.

From these experiments I was led to conclude, that the principal action of the muriate of barytes is on the brain; but in the first the pulse was feeble and intermitting; in the second, although the artificial respiration was made with the greatest care, the circulation could not be maintained more than a few minutes. These circumstances led me to suspect, that although this poison operates principally on the brain, it operates, in some degree, on the heart also. Farther experiments confirmed this suspicion. In some of them the pulse soon became so feeble, that it could be scarcely felt; and its intermissions were more frequent; but in all cases the heart continued to act after respiration had ceased; and the cessation of the functions of the brain was therefore always the immediate cause of death. When I employed artificial respiration, after death had apparently taken place, I seldom was able to prolong the heart's action beyond a few minutes. In one case only it was maintained for three quarters of an hour.

but in some degree on the heart.

hour. I never by these means succeeded in restoring the animal to life, although the experiments were made with the greatest care, and in a warm temperature. In some instances, after the artificial respiration had been kept up for some time, there were signs of the functions of the brain being in some degree restored; but the pulse notwithstanding continued to diminish in strength and frequency, and ultimately ceased. I shall detail one of these experiments, as it serves to illustrate the double action of this poison on the nervous and vascular systems.

Exp 7. Action of the muriate of barytes on the nervous and vascular system illustrated.

Experiment 7. Some muriate of barytes was applied to a wound in the side of a rabbit. The usual symptoms took place, and at the end of an hour the animal was apparently dead; but the heart still continued to contract. He was placed in a temperature of 80°, and a tube being introduced into the nostril, the lungs were artificially inflated about thirty-six times in a minute.

When the artificial respiration had been maintained for four minutes, he appeared to be recovering; he breathed voluntarily one hundred times in a minute, and showed signs of sensibility. The artificial respiration was discontinued. The voluntary respiration continued about nine minutes, when it had ceased, and the animal was again apparently dead; but the pulse continued strong and frequent. The lungs were again artificially inflated. At the end of four minutes the animal once more breathed voluntarily one hundred times in a minute, and repeatedly moved his limbs and eyelids. The pulse became slower and more feeble.

In a few minutes the voluntary respiration again ceased, and the artificial respiration was resumed. The pulse had fallen to one hundred, and was feeble. The animal again breathed voluntarily; but he ceased to do so at the end of five minutes. The lungs were inflated as before; but he did not give any sign of life, nor was the pulse felt afterward. On opening the thorax, his heart was found to have entirely ceased acting.

A probe having been introduced into the spinal marrow, it was found, that by means of the Voltaic battery powerful contractions might be excited, not only of the voluntary muscles, but also of the heart and intestines; from which it may be inferred, that the muriate of barytes, like arsenic, affects the circu-

Like arsenic, it renders the

heart insensible to the stimulus of the blood.

circulation by rendering the heart insensible to the stimulus of the blood, and not by destroying altogether the power of muscular contraction.

It affects the stomach, but less than arsenic.

The muriate of barytes affects the stomach, but in a less degree than arsenic. It operates as an emetic in animals that are capable of vomiting; but sooner when taken internally, than when applied to a wound. In general, but not constantly, there are marks of inflammation of the inner membrane of the stomach, but not of the intestine. In many instances there is a thin layer of dark coloured coagulum of blood lining the whole inner surface of the stomach, and adhering very closely to it, so as to have a good deal of the appearance of a slough; and this is independent of vomiting, as, where I met with it, it occurred in rabbits.

The same circumstances, from which it may be inferred, that arsenic does not produce its deleterious effects until it has passed into the circulation, leads to the same conclusion with regard to the muriate of barytes.

V. On the Effects of the Emetic Tartar.

Emetic tartar has similar effects.

The effects of the emetic tartar so much resemble those of arsenic and of muriate of barytes in essential circumstances, that it would be needless to enter into a detail of the individual experiments made with this poison.

Applied to a wound.

When applied to a wound in animals, which are capable of vomiting, it usually, but not constantly, operates very speedily as an emetic; otherwise I have found no material difference in the symptoms produced in the different species of animals, which I have been in the habit of employing as the subjects of experiment. The symptoms are paralysis, drowsiness, and at last complete insensibility; the pulse becomes feeble; the heart continues to act after apparent death; its action may be maintained by means of artificial respiration, but never for a longer period than a few minutes: so that it appears, that this poison acts on the heart as well as on the brain; but that its principal action is on the latter. Both the voluntary and involuntary muscles may be made to contract after death, by means of Voltaic electricity. The stomach sometimes bears the marks of inflammation; but at other times it has its natural appearance. I have never seen any appearance of inflammation of the intestines. The length of time which elapses from the application of the poison to the death of the animal varies. In some instances it is not more than three quarters of an hour; but in others it is two or three hours, or even longer.

Acts in the same manner internally.

When a solution of emetic tartar was injected into the stomach of a rabbit, the same symptoms took place as when it was applied to a wound.

VI. On the Effects of the Corrosive Sublimate.

Effects of muriate of mercury.

When this poison is taken internally in very small and repeated doses, it is absorbed into the circulation, and produces on the system those peculiar effects, which are produced by other preparations of mercury. If it passes into the circulation in larger quantity, it excites inflammation of some part of the alimentary canal, the termination of which may vary accordingly as it exists in a greater or less degree. When taken in a larger quantity still, it occasions death in a very short space of time. I had found, that, if applied to a wounded surface, it produced a slough of the part to which it was applied, without occasioning any affection of the general system. This led me to conclude, that the effects of it, taken internally, and in a large quantity, depended on its local action on the stomach, and were not connected with the absorption of it into the circulation. The following experiments appear to confirm this opinion.

They depend on its local action.

Exp. 8. Administered internally to a rabbit.

Experiment 8. Six grains of corrosive sublimate, dissolved in six drams of distilled water, were injected into the stomach of a rabbit, by means of an elastic gum tube. No immediate symptoms followed the injection; the animal made no expression of pain; but in three minutes he became insensible; was convulsed; and in four minutes and a half from the time of the injection being made, he died. Tremulous contractions of the voluntary muscles continued for some time afterward. On opening the thorax, the heart was found to have entirely ceased acting, and the blood in the cavities of the left side was of a scarlet colour. The stomach was much distended. The pyloric and cardiac portions were separated from each other by a strong muscular contraction. The contents of the former were firm and solid, and in every respect resembled the usual contents of the stomach; while those of the cardiac portion consisted of the food of the animal much diluted by fluid; so that the solution, which had been injected, appeared to be confined to the cardiac portion of the stomach, and to be prevented entering the pyloric portion by the muscular contraction in the centre.

In the pyloric portion of the stomach the mucous membrane had its natural appearance; but in the cardiac portion it was of a dark gray colour, was readily torn and peeled off; and in some parts its texture was completely destroyed, so that it appeared like a pulp, on removing which the muscular and peritoneal coats were exposed.

Experiment repeated.

The repetition of the experiment was attended with similar results. The alteration of the texture of the internal membrane appears to have been occasioned by its being chemically acted on by the corrosive sublimate injected into it. When the injection is made into the stomach of a dead rabbit, precisely the same effects are produced, except that, as the middle contraction is here wanting, the appearances are not confined in the same degree to the cardiac portion.

Similar effects on the stomach of a dead rabbit.

Exp. 9. Muriate of mercury given to a cat.

Experiment 9. A scruple of corrosive sublimate, dissolved in six drams of distilled water, was injected into the stomach of a full grown cat. For the first five minutes no symptoms were produced. After this, the poison operated twice as an emetic. The animal appeared restless, and made expression of pain in the abdomen. He gradually became insensible, and lay on one side motionless, with the pupils of the eyes dilated. The respiration was laborious, and the pulse could not be felt. Twenty-five minutes after the poison was injected, there was a convulsive action of the voluntary muscles, and death ensued. On opening the thorax immediately afterward, the heart was seen still contracting, but very feebly.

Appearances on dissection.

The stomach was found perfectly empty and contracted. The mucous membrane was every where of a dark gray colour. It had lost its natural texture, and was readily torn and separated from the muscular coat. The internal membrane of the duodenum had a similar appearance, but in a less degree, for nearly three inches from the pylorus. In the situation of the pylorus the effects of the poison were less apparent than in any other part.

The particular state of the internal membrane of the stomach, in this experiment as well as in the last, appears to have been occasioned by the chemical action of the poison on it. When I injected a solution of corrosive sublimate into the stomach of a dead cat, and retained it there for a few minutes, a similar alteration of the texture of the internal membrane took place; but it assumed a lighter gray colour. The difference of colour may be explained by the vessels in the one case being empty, and in the other case being distended with blood at the time of the injection being made.

Effects on the stomach of a dead cat similar.

The destruction of the substance of the internal membrane of the stomach precludes the idea of the poison having been absorbed into the circulation. We must conclude, that death was the consequence of the chemical action of the poison on the stomach. This organ, however, is not directly necessary to life, since its functions, under certain circumstances, are suspended for hours, or even for days, without death being produced. Although the stomach was the part primarily affected, the immediate cause of death must be looked for in the cessation of the functions of one or more of those organs, the constant action of which is necessary to life. From the scarlet colour of the blood in the left side of the heart, in the experiment on the rabbit, we may conclude, that the functions of the lungs were not affected; but the affection of the heart and brain is proved by the convulsions, the insensibility, the affection of the pulse in both experiments, and the sudden cessation of the heart's action in the first; and we may therefore be justified in concluding, that the immediate cause of death was in both of these organs. As the effects produced appear to have been independent of absorption, we may presume, that the heart, as well as the brain, was acted on through the medium of the nerves.

The muriate acts directly on the stomach;

but produces death by indirectly destroying the functions of the heart and brain.

That a sudden and violent injury of the stomach should be capable of thus speedily proving fatal, is not surprising, when we consider the powerful sympathy between it and the organs on which life more immediately depends, and the existence of which many circumstances in disease daily demonstrate to us.

VII. The facts which have been stated appear to lead to the

General infe-

following

rences respecting the action of these mineral poisons.

following conclusions respecting the action of the mineral poisons, which were employed in the foregoing experiments.

1. Arsenic, the emetic tartar, and the muriate of barytes, do not produce their deleterious effects until they have passed into the circulation.

2. All of these poisons occasion disorder of the functions of the heart, brain, and alimentary canal; but they do not all affect these organs in the same relative degree.

3. Arsenic operates on the alimentary canal in a greater degree than either the emetic tartar, or the muriate of barytes. The heart is affected more by arsenic than by the emetic tartar, and more by this last, than by the muriate of barytes.

4. The corrosive sublimate, when taken internally in large quantity, occasions death by acting chemically on the mucous membrane of the stomach, so as to destroy its texture; the organs more immediately necessary to life being affected in consequence of their sympathy with the stomach.

Mineral and vegetable poisons compared.

In making the comparison between them, we observe, that the effects of mineral, are less simple than those of the generality of vegetable poisons; and when once an animal is affected by the former, there is much less chance of his recovery, than when he is affected by the latter.

IV.

IV. *On the Vegetation of high Mountains, translated from a Paper of Mr.* RAMOND'S *in the Annales du Museum, V.* 4, *p.* 395. *By* RICHARD ANTHONY SALISBURY, *Esq. F. R. S.* &c*.

Vigorous vegetation on mountains in temperate regions.

AN observing gardener, on ascending the high mountains of our temperate region, is immediately struck with the vigour and luxurious appearance of their vegetation. The plants he has seen in the adjacent plains are changed in size, aspect, and form, so that he hardly recognises the most common. Their stems are elevated, their flowers larger, even the leaves of the trees have acquired a size, which makes him doubt the identity of the species. The woods are more impenetrable, the turf of the downs closer, and a green more lively, fresh, and brilliant, colours every thing, from the depths of the valley, up to those heights, where the eye can discern nothing but naked rocks and eternal snows*.

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* Hort. Trans. vol. I, appendix, p. 15.

Growth of vegetables rapid.

Thus, endowed with a vigour elsewhere unknown, vegetables there hasten with increased energy through the various periods of their existence. Time, which to them moves slowly in the plains, in the mountains flies. There, every thing is done rapidly; meteors dart after each other, and the air is in perpetual agitation. From all these controlling causes, acting together in full force, germination, florescence, and fructification take place almost simultaneously. Sometimes, with a wind blowing from the south, with a heavy shower, or with a scorching sun, the face of the meadows, downs, and forests, in a moment changes, and the whole of a particular species seems to vanish; in fact, there, every fine day is a spring to some particular assemblage of vegetables, or to some of the inaccessible heights in which they grow.

Their localities more distinct.

To this picture, another succeeds. If we examine the mountains and vallies, every place has its peculiar soil, every different elevation its peculiar climate, and each of them its characteristic vegetables. In the plains, these vegetable assemblages occupy vast spaces, the limits of which are too extensive, and indeterminate, to be easily perceived. On the contrary, in the mountains, they are confined to narrow limits, which the eye often takes in at one view. In a gentle rising extended between two dales, in a pile of rocks, or in a cliff, which the traveller ascends in a few moments, he finds the perpetual barriers of those productions, which nature has been pleased to separate.

Particular effect of height.

Among the various causes of these separations, one seems to reign predominant over all others; this is, elevation above the level of the sea. In every 100 inches in height, the temperature falls about half a degree of our thermometers. After that degree of cold, which generally puts a stop to all vegetation, an eternal frost prevails on the summit of these Alps, as at the poles; and every 100 metres of vertical elevation, corresponds nearly to one degree of the distance at which the mountain is placed from the pole.

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* The first part of this sentence rather applies to purely mountainous plants, such as *aster alpinus*, *viola grandiflora*, *aquilegia vulgaris*, &c., than to all vegetables indiscriminately; the latter part I should explain by saying, that the foliage of the trees was rather diminished in the dry plains at the base of the *Pyrenees*, than enlarged by mere elevation, but, along with elevation, to a certain extent, perpetual moisture and food are washed down to their roots; and such a situation in *France*, is probably the aboriginal one of the trees in question. *Sec.*

100 yards nearly equivalent to a degree of latitude.

Two causes of the distribution of vegetables.

By this scale, the various phenomena of different climates in our globe may be easily understood; circumstances may differ, but the general results will be nearly the same. While the increase of cold is accompanied by a diminution of the column of air, it is also affected by the obliquity of the rays of the sun, and the distribution of vegetables, in all alpine countries, depends principally on these two causes.

Trees.

Thus, in the *Swiss Alps,* and *Pyrenees,* trees cease to grow at about 2400 or 2500 metres of actual elevation, as they do about the 70th degree of north latitude; and that circle these gigantic vegetables occupy, is divided into several less bounds, which have each their peculiar characteristics. At the foot of the mountain we find the *oak:* in the middle region the *beech:* above these the *fir* and *yew* succeed, which soon give place to the *pine* (Pinus *sylvestris* L.). Along with this last mentioned tree, in the *Swiss Alps* the *larch* and *cembro* (Pinus *cembra* L.) also grow wild, which are unknown in the *Pyrenees.* The *cedar of Lebanus* would probably thrive as well on these mountains, as on those of Asia, had it been fixed there; but such is still the mystery of the original dissemination of vegetables, that Nature seems by turns, indifferent to the similitude of places, or to the distance between them; sometimes bringing together in the same climate, plants of the most distant countries; and sometimes denying this conformity of vegetables to regions exactly alike, both in soil and temperature.

Rhododendron.

In this zone of trees, the *rhododendron ferrugineum* L. a little shrub peculiar to the mountains of *Europe* solely, is very abundant. It never descends into the plains, and can hardly be cultivated in a garden, demanding its native air, soil, water, nay snows, and even there only occupies particular spots. Nothing is more beautiful when in flower, but nothing is more untractable. In the *Pyrenees* it first appears at exactly 1600 metres of elevation, stopping as precisely at 2600 metres, and within these limits, is so abundant and vigorous, that it would be as difficult to extirpate it there, as it is to cultivate it elsewhere*.

Juniper.

The *juniper* traverses far beyond this circle, up to the elevation of 2900 metres, but this shrub, as it ascends, gradually loses the habit and appearance, which distinguish it in our plains: there, it resembles the *juniper* of *Sweden* and *Lapland*, with a low spreading stem, prostrate on the ground, seeking an asylum, as it were, by instinct on those sides of the rocks exposed to the south or west, against which it spreads out its branches into an espalier, with a regularity which art can seldom attain†.

Annuals scarcely found at a certain height.

In a more elevated region, we find the rigour of the climate will not permit the existence of any shrub whatever, which the first snows do not entirely cover. Still higher, even this shelter is insufficient, and nothing but a few herbs, with perennial roots actually under the earth, subsist. Nature has almost entirely banished from such places annual plants; where the whole summer is reduced to a few days, nay, sometimes a few hours; where often a storm of wind, or dripping fog, will destroy the flowers which have scarcely blossomed, and, bringing back winter, terminate the year.

Hardy perennials.

On the contrary, hardly any elevation seems to stop the progress of some perennials, which, on the approach of severe cold, shelter themselves under the double protection of the earth and snow, forming their buds underground, and springing up the first fine day of the succeeding year. Their duration exhausts the chances of all [illegible]s and seasons, till, sooner or later, they also ripen seed, by which they are multiplied.

Plants at the height of 3278 yards.

Thus the vegetable zone of our alps has in fact no other limits, than those of the earth or soil covering them. The *Pic du Midi*, which I have ascended 26 times, is 3000 metres above the level of the sea, but I never once found the thermometer there rise to the temperate point. Yet, on a nearly bare rock, I have there gathered as many as 48 species of vegetables, excluding cryptogamous plants: of these, one only, which perhaps I may never at 3552 yards,

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* No shrub is more plentiful, or easily cultivated in the gardens about *London*, if planted in light sandy peat under a rock, or north-west wall, and watered plentifully in dry weather.—*Sec.*

† Two distinct species are probably here confounded, an opinion in which I was confirmed by the late Mr. Dryander.—*Sec.*

at 3825 yards.

find again, was annual. At *Nieuville*, a place 250 metres higher than the *Pic du Midi*, where the thermometer in summer never rises to more than 8 degrees, I have, in five journies, collected 12 different perennials. On the top of *Mont Perdu*, at an elevation of 3500 metres, even in the bosom of permanent snows, but on rocks the sloping situation of which had cleared them of snow, I have seen six different plants very vigorous. Here, in one of the hottest days of a summer remarkable for its heat, the thermometer only rosé to 5·5° above the point of congelation, and it undoubtedly falls in winter to 25 or 30: nor is it certain, that those 6 plants, found in a season which melted more snow than usual, are regularly uncovered every year. Besides, I have seen some of them on the borders of the perpetual snow, with only half of their stems exposed and vegetating, the other half buried in it*, and it is probable, that many of them do not see the light ten times in a century, running through the whole course of their vegetation in a few short weeks, and doomed afterwards to sleep through a winter of many years.

These plants confined to mountains, or the vicinity of the poles.

Plants subjected to so singular a mode of existence are not among the species which grow in the plains of our temperate regions: they belong exclusively to such as grow on the summits of mountains, or near the poles. *Norway*, *Lapland*, and *Greenland*; furnish plants analogous to those of the *Swiss Alps* and *Pyrenees*; but few, or possibly none of them, are seen in *Siberia*, *Kamschatka*, or even in the polar regions of *America*. One would hardly have supposed so great a diversity of vegetable productions in countries so much alike and near each other, nor on the other hand, so great a conformity as exists among the plants of these countries, and the plants of some alpine regions distant from them 40 degrees.

Plants not disseminated in parallel latitudes.

In fact, we learn from actual observation, that the dissemination of vegetables is not always regulated in parallel distances from the equator; that if a certain number of plants, confined by their constitution to a peculiar climate, are to be found to a certain distance under the same latitudes, many others, on the contrary, have been scattered over different countries in the direction of their meridians. Towards the south, *America, Africa*, and *Asia*; towards the north, *Europe, Asia*, and *America*, are far from producing the same vegetables under the same parallels; while many plants, growing wild in each of these grand divisions of the globe, brave every obstacle opposed to them by a diversity of climate, and propagate themselves in a geographical direction quite contrary to that which a similar climate would confine them to.

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* A similar case occurred in a vine at *Chapel Allerton*, planted in the open air, at some distance from the stove; a branch of which, however, being introduced into the stove early in *January*, was loaded with clusters of grapes, before any of the buds exposed to the open air, shot out.—

Progress of curious plants.

Thus, for example, many of the curious plants of *Sardinia, Sicily*, and *Italy*, mount up the *Swiss Alps*, and then descend again into the lower parts of *Germany*, without being allured by our fine climate to *France*. Thus, likewise, the *Pyrenees* receive from *Spain* a great number of the plants of *Barbary*, scattering them over the western provinces of *France*. The *merendera*, which grows in the north of *Africa*, is found in *Andalusia, Castile, Arragon*; when crossing the *Pyrenees* it descends as far as the *Landes de Bourdeaux*. The *narcissus bulbocodium**, and *hyacinthus serotinus*, grow wild in the same places, and follow the same route. The *anthericum bicolorum* of *Algiers*, traverses the same chain of mountains, and arrives in *Anjou*. The *scilla umbellata* and *crocus nudiflorus*, have migrated from the *Pyrenees* even into *England*. Yet not one of the above mentioned vegetables have been disseminated laterally, to meet those southern ones which have crossed the *Swiss Alps*.

This most striking in the valleys of the Pyrenees.

But it is in the great valleys of the *Pyrenees*, extending from north to south that these vegetable galaxies become most striking and singular. The *dianthus superbus* runs through the whole valley of *Campan* and *Gavarnie*, without ever entering any

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Three species confounded by the author.

* Here the celebrated author confounds three very distinct species. The plant of the *Pyrenees* is the *N. Bulbocodium* L. with erect leaves, very hardy, and brought forced to *Covent-garden* in abundance every spring. The plant of *Barbary* and *Andalusia*, which I received from the late professor Broussonet, is more dwarfish, with leaves spreading flat on the ground, and so tender, that it will only live here through winter, in very warm sandy soils, close to a wall. The plant of *Castile* grows also near *Oporto*, and differs from both the others, in having a six-lobed plaited crown, with very narrow leaves; it is not very tender, but requires a dry sandy soil. *Sec.*

Verbascum Myconi.

Box.

of the side ones. The *verbascum Myconi*, that beautiful and scarce plant, which does not belong either to the genus in which LINNÆUS has placed it, or perhaps to any natural order yet defined, and which has so exotic an appearance, that it distinguishes itself like the *king fisher*, among our indigenous birds, invariably keeps to the same direction. Nothing is more abundant in all the great valleys of the *Pyrenees*, in every soil and exposition: yet the very same soil and exposition never attract it to any of the collateral ones. I could cite a multitude of similar examples, but it is sufficient at present, to mention one more, the *box tree*. This shrub, so very robust, is affected by elevation like the most delicate ones. At the base of the *Pyrenees*, both on the *French* and *Spanish* side, it covers every hill: thence it enters the great valleys, running from the north-east towards the south, but never quits them; in vain do the numerous branches of these valleys offer it an asylum; passing their openings, it keeps to its first direction, stopping on the crest of the chain at about 2000 metres above the level of the sea, and appearing again on the other side at a similar elevation, and in a similar direction, from which it never deviates.

Local influence striking in mountainous countries,

but even here modified by man.

Thus it is, that in high mountainous countries we discover the strongest traces of the original design of nature; there, each order of vegetables is confined within narrower bounds; there, local influence more powerfully resists every other. Nevertheless, the lapse of ages, and especially the presence of man, has here introduced many modifications; for, in traversing the immense deserts of these high mountains, among the rare plants which form their herbage, some few of the commonest here and there occur. If the verdure takes a deeper tint than usual, contrasted with the gayer colour of the alpine turf, the ruins of a hut, or a rock blackened by smoke, explain the mystery. Around these asylums of man, we find naturalized the common *mallow*, *nettle*, *chickweed*, common *dock*. A shepherd had possibly sojourned here some weeks, and, hither, in driving his flocks here, had also attracted without knowing it, the birds, the insects, the seeds of the plants of his lowland cot. He may possibly never return, but these wild spots have received in an instant the indelible impression of his footsteps; so much weight has a being of his importance in the scale of nature.

Woods destroyed by him.

In other places, by destruction he has signalized his presence. Before he approached the mountains, the immense forests which covered their bases have fallen under his axe, for woods are not the abodes of man; he avoids the circuitous paths of so vast a labyrinth, suspecting danger under their shades; he there mourns the absent sun, an object which every day renovates his delight; and therefore it is seldom that he penetrates a forest, without fire and sword in hand.

and with them woodland plants.

Accordingly the seeds of woodland plants become dormant in a soil now dried by the sun and wind, and no longer suitable to their germinating. Other vegetables take their places, the climate itself changing; for the temperature rises, the rains are less frequent, but more copious, the winds more inconstant and impetuous, deep gullies are formed in the sides of the acclivities by torrents, and rocks are deprived of the earth which covered them, and, at the same time, of the plants which ornamented them, by falls of immense loads of melting snow; thus the face of the globe, where man inhabits, is more changed in one century, than in twenty where he is absent.

The horticulturist should visit alpine countries as well as the geologist.

After all, in Alpine countries, the different soils, and their productions, retain most of their aboriginal character: there, the primitive distribution of vegetables has been least disturbed; their localities can be easily traced, the influence of the air is most perceptible; there, the contiguity of objects exhibiting more forcibly their similitudes and dissimilitudes, the eye of the observer takes in, at one glance, every trait which is interesting; and if it is necessary for the geologist to visit these grand chains of mountains, to study the structure of the earth and those catastrophes, which have imprinted its present form, it is still more so for the horticulturist, who wishes to penetrate the mysteries of the primary dissemination of vegetables and their subsequent propagation, hoping thence to derive hints for their successful cultivation and improvement, in the paradise surrounding his dwelling.

IV.

Description of a Bank for Alpine *Plants, by Monsieur* THOUIN, *abridged from his Paper in the* Annales du Museum, *V.* 6, p. 183. *By* RICHARD ANTHONY SALISBURY, *Esq. F. R. S. &c.*[*]

Bank for the culture of alpine plants, in the botanical garden at Paris.

PLANTS from alpine and frozen countries are cultivated in the *Jardin des Plantes* at *Paris*, in a *bank*, 60 feet long, placed against the wall of a terrace, 10 feet high, which faces the south-east so much, that the sun ceases to shine upon it between 10 and 11, A. M. This bank is divided into 5 steps, 1 foot wide, by nailing planks of oak, 10 inches deep, to the top of as many rows of strong posts, charred at the bottom, and driven firmly into the ground; the taller posts are still further secured in their places by cross bars let into the wall.

Through the whole length of this *bank* runs a ditch, 2 feet deep, but sloping gradually towards the front up to 9 inches in height, under the general level of the ground; and in making this ditch, its sides were plastered 6 inches thick with mortar of brick mould and chopped straw; filling up all the cracks which appeared during the week it was left exposed to the air. After nailing the planks to the posts, the natural soil, which is of a light nature, was thrown into the hollow up to within about a foot of the surface of the slope, above which it was filled with sandy peat, such as *ling* and *heaths* grow in, passed through a screen. My reason for using all these precautions was to prevent the water necessary for the health of those *alpine* plants in summer, running off too quickly into a bed of dry gravel underneath; in a naturally moist soil, this expense and trouble may be saved.

Seeds sown in it.

I have sown on this *bank* the seeds received not only from the *Alps*, but several other frozen regions; for it is probable, that the elevation of the atmosphere near the *poles* corresponds with that of the highest mountains in *France*, rising gradually toward the equator; nor is this consideration so foreign to the business of a gardener in naturalizing vegetables, as might be at first supposed.

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[*] Horti. Trans. vol. I, appendix, p. 24.

Roots of all the *alpine* plants I could collect, have also been planted in this *bank*, and they thrive much better than when cultivated in pots on a stage, however open or airy, so that most of the following have greatly increased both by seeds and roots. *Moehringia muscosa, viola biflora, androsace carnea,* and *lactea, soldanella alpina, primula farinosa,** *tussilago, alpina, artemisia glacialis, salix myrsinites, retusa,* and *reticulata.*

Roots planted in it.

Catalogue.

The culture they require is, 1st, to keep the *bank* carefully weeded: 2dly, to reduce within bounds many that grow and spread rapidly so as to exclude others: 3dly, to dig and lighten the surface frequently, that it may absorb air and water more readily: 4thly, to add three inches in depth of fresh sandy peat every year, in place of the old, which soon loses its *humus,* or nutritious part: 5thly, in giving the plants, at a certain season, not only daily, but hourly waterings; but this being one of the most important points, I shall enlarge more fully upon it.

Management.

Almost all *alpine* plants are of humble stature, growing on steep declivities of rocks in a layer of *humus* or vegetable earth, formed by the decomposition of *jungermannias, lichens,* and *mosses.* The greater part of the year, they are covered with a bed of snow, which only begins to melt at stated periods of the day, after the rays of the sun have acquired great force. Then only do these *alpine* plants awaken from torpidity, exhaling quickly in this light black soil the moisture which they have absorbed during the night; but the returning sun, which excites them to action, also melts the snow above, the waters of which trickling down to their roots, give immediate refreshment. The sun disappearing, these little vegetables are no longer exhausted, and a continuance of moisture would even be hurtful; accordingly the snow resuming its solid consistence with the cold of the night, this natural irrigation ceases, with a degree of exactness, that the most careful gardener cannot perform.

Alpine plants naturally watered by the melting snow.

From the above remarks, it will easily be deduced, that *alpine* plants should have no water at all during winter and dank moist weather: on the contrary, that they should be kept per-

Artificial watering.

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* I have constantly found this plant growing wild in wet meadows that are seldom dry even in summer, at the foot of the mountains, and even in bogs. *Sec.*

petually moist during hot sunshine, by water dribbling through the soil to their roots, without wetting their leaves, which, immediately evaporating by the heat, will cool the air just above them. In fact, it is only by a close imitation of the process of nature, that these vegetables of cold regions can be successfully cultivated in botanic gardens.

They must be covered from frost.

The last essential point relative to *alpine* plants is to cover them up on the approach of frost: this may appear a strange precaution to some, but when winter commences in their native soil, being immediately covered with snow to the depth of seven inches, they never feel a greater degree of cold than that of the freezing point, the soil itself being hardly frozen. The best covering is that of *fern, pteris aquilina,* which does not absorb moisture so quickly as most other sorts of haulm.

V.

On a Periscopic Camera Obscura and Microscope. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S. From the Philosophical Transactions for* 1812, *p.* 370.

Periscopic improvement of the camera.

ALTHOUGH the views which I originally had of the advantage to be derived from the periscopic construction of spectacles*, naturally suggested to me a corresponding improvement in the *camera obscura,* by substituting a meniscus for the double convex lens, I have hitherto deferred making it known to others, except as a subject of occasional conversation.

The mathematical consideration applied to spectacles is not with convenience applicable to the cam. obs.

Since in vision with spectacles, as in common vision, the pencil of rays received by the eye in each direction is small, the superiority of that form of glass, which disposes all parts of it most nearly at right angles with the visual ray, admits of distinct demonstration; but with respect to the camera obscura, where the portion of lens requisite for sufficient illumination, is of considerable magnitude, although it is evident that some improvement may be made in the distinctness of oblique images on the same principles, yet as the focus of oblique rays is far from being a definite point, the degree in which it may be improved is not a fit subject of mathematical investigation.

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* Phil. Magaz. Vol. XVII. Nicholson's Journal, VII. 148.

I have therefore had recourse to experiments, in order to determine by what construction the field of distinct representation may be most extended; and I trust the result will be acceptable to this society. I shall take the same opportunity to describe an improvement in the construction of the simple microscope, which may also be termed periscopic, as the object of it is to gain an extension of the field of view, upon the same principles as in the preceding instances, namely, by occasioning all pencils to pass as nearly as may be at right angles to the surfaces of the lens. The mode, however, in which this is effected is apparently somewhat different in the practical execution.

Experiment preferable.

Periscopic microscope.

In the common *camera obscura,* where the images of distant objects are formed on a plane surface to which the lens is parallel, if the surfaces of the lens be both convex, and equally curved (as in fig. 1, Pl. I); and if the distance of the lens be such, that the images formed in the direction of its axis CF be most distinct, then the images of lateral objects are indistinct in a greater or less degree, accordingly as they are more or less remote from the axis. The causes of this indistinctness may be considered as twofold; for in the first place, all parts of the plane, excepting the central point, are at a greater distance from the centre of the lens, than its principal focus; and secondly, the point *f*, to which any pencil of parallel rays, passing obliquely through the lens, are made to converge, is less distant than the principal focus. On this account, it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre. The want of distinctness, however, is even then only diminished in degree, but is not remedied.

In the common cam. obs. the side images are indistinct,

because the plane is more distant than the principal focus,

and the oblique pencils have a focus still shorter.

The construction, by which I propose to obviate this defect, is represented in the second figure, in which are seen the essential parts of a periscopic camera in their due proportion to each other. The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed that its concavity is presented to the objects, and its convexity toward the plane on which the images are formed. The aper-

New construction. With a meniscus lens concave towards the object, and an aperture at a distance from the concave face.

aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted.

Statement of its advantages.

The advantage of this construction over the common camera obscura is such, that no one who makes the comparison, can doubt of its superiority ; but the causes of this may require some explanation. It has been already observed, that by the common lens, any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius ; and hence are made to converge to a point so distant; that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed.

The oblique pencils have a longer focus than the principal focus.

The aperture in this construction represents by inversion the pupil of the eye in the p. spectacles.

In the use of spectacles by long-sighted persons, the course of the rays in the opposite direction is so precisely similar, that the same figure might serve to illustrate the advantages of the periscopic construction. For the purpose of seeing the extended page of a book (as at AB) with least fatigue to the eye, that form of lens will be most beneficial, which renders the rays received from each part of its surface parallel ; and this is effected by the exact counterpart to the preceding arrangement ; for in this case the opening E represents the place of the eye receiving parallel rays from the lens in each direction, instead of transmitting them from a distance towards it.

Limit of ad

There is, however, this difference between the two cases,

that

vantage in the camera.

that in the camera obscura, a much larger portion of the lens is required to conspire in giving a distinct image of any one object; so that the conformation best adapted for lateral objects would not be consistent with distinctness at the centre; and hence arises a limit to the application of the principle. On the common construction, the whole lens is so formed as to give brilliancy and distinctness at the centre alone, without regard to lateral objects. In adopting such a deviation from the customary form, as I propose, in favour of a more extended view, some diminution of the aperture is required in order to preserve the desired distinctness at the centre. In my endeavours to ascertain the most eligible form of meniscus for this purpose, I have assumed sixty degrees to be the field of view required. But when so large a field is not wanted, then a lens that is less curved will be preferable; and the proportion of the radii must be varied according to the angular extent intended to be included.

Best construction for a large field of view.

Diagram explained by which the radii of lenses are settled.

For the purpose of estimating by what combination of radii any required focal length may be given to a meniscus, I have contrived a diagram by which very much labour of computation may be saved, as a very near result may be obtained by mere inspection. This contrivance is founded on the well known formula for the focal length of any lens $F = \frac{mrR}{R \pm r}$: m being a certain multiple obtained by dividing the sine of refraction by the difference of the sines of incidence and refraction. Hence, in applying this formula to the meniscus, $F : R :: mr : R-r$. In fig. 3, lines expressive of these quantities are so arranged, that by assuming any point F corresponding to the focal length desired, and drawing a line FR through a point R indicating any supposed length of the greater radius, the corresponding length of the other radius will be found where the line drawn intersects the middle line in the diagram.

In laying down these lines, the length and position of AF and AR were assumed at pleasure; and they were divided into any number of equal parts. But the position and length of the middle line Ax was adapted with care to the refractive power of plate glass in the following manner. Since $m = \frac{1}{1{,}505-1} = 1{,}98$, a line BC was drawn from the point 10 in the line AR, parallel to AF, and equal to 19,8 divisions of the primary lines; so that if r be $=10$. then the line BC $= mr$. The distance AC being then divided into ten equal parts, with their subdivisions, afforded the means of continuing the same scale to any desired length. Since the first line BC was laid down parallel to AF, and equal to mr, any other lines drawn through corresponding numbers 7 and 7, 8 and 8, &c. will be also parallel, and by preserving due proportion will correctly represent mr. Hence in all positions of the line FR, the same similarity of triangles obtains, and the same proportion of $F : R :: mr : R-r$; and consequently the focal length, corresponding to any assumed radii, is truly ascertained.

For the purpose of duly proportioning the curvatures of flint glass, a second line Ay might be laid down in a mode similar to the preceding, by adapting the multiple $m = \frac{1}{1,58-1} = \frac{9}{18}$ to the different density of this glass.

Periscopic simple microscope.

With respect to the construction of a microscope on periscopic principles, I believe the contrivance to be equally new with the former, and equally advantageous. The great desideratum in employing high magnifiers is sufficiency of light; and it is accordingly expedient to make the aperture of the little lens, as large as is consistent with distinct vision. But if the object to be viewed, is of such magnitude as to appear under an angle of several degrees on each side of the centre, the requisite distinctness cannot be given to the whole surface by a common lens, in consequence of the confusion occasioned by oblique incidence of the lateral rays, excepting by means of a very small aperture, and proportionable diminution of light.

Two plano-convex lenses placed face to face with a central aperture in a plate between them.

In order to remedy this inconvenience, I conceived that the perforated metal, which limits the aperture of the lens, might be placed with advantage in its centre; and accordingly I procured two plano-convex lenses ground to the same radius, and applying their plane surfaces on opposite sides of the same aperture in a thin piece of metal (as is represented by a section, fig. 4), I produced the desired effect; having virtually a double convex lens so contrived, that the passage of oblique pencils was at right angles with its surfaces, as well as the central pencil. With a lens so constructed, the perforation

Dimensions.

that appeared to give the most perfect distinctness was about one-fifth part of the focal length in diameter; and when such an aperture is well centered, the visible field is at least as much as twenty degrees in diameter. It is true, that a portion of light is lost by doubling the number of surfaces; but this is more than compensated by the greater aperture, which, under these circumstances, is compatible with distinct vision.

Application of periscopic principles to the camera lucida.

Beside the foregoing instances of the adaptation of periscopic principles, I should not omit to notice their application to the camera lucida; as there is one variety in its form, that was not noticed in the description which I originally gave of that instrument*.

In drawing, by means of the camera lucida, distant objects are seen by rays twice reflected (*d*, fig. 5), at the same time and in the same direction that rays (*e*) are received from the paper and pencil by the naked eye. The two reflections are effected in the interior of a four-sided glass prism, at two posterior surfaces inclined to each other at an angle of 135 degrees. In the construction formerly described, the two other surfaces of the prism are both plane, through which the rays are simply transmitted at their entrance and exit. But since an eye that is adjusted for seeing the paper and pencil, which are at a short distance, cannot see more distant objects distinctly without the use of a concave glass, it may be assisted in that respect by a due degree of concavity given to either, or to both the transmitting surfaces of the prism. It is, however, to the upper surface alone that this concavity is given; for since the eye is then situated on the side toward the centre of curvature, it receives all the benefit that is proposed from the periscopic principles.

VI.

Practical Experiments on hardening Steel. By Mr. E. Lydiatt, *Lecturer on metallurgy, and the mechanic Arts, &c. In a letter from the Author.* *

To W. Nicholson, Esq.

Sir,

Introduction.

THE desire I feel to be instrumental in promoting the cause of science and truth, makes me regret that indispensable avocations prevent me from communicating much information to your valuable journal, that would stand a chance, at least, of being useful to many of your readers."

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* Nicholson's Journal, XVII, p. 1. Phil. Magaz. XXVII, p. 343.

Common Tenacity of metals deferred.

To this circumstance alone, is to be referred the delay of my promised communication on the tenacity of the different metals. The time necessarily required to complete experiments on this subject, I have not yet been able to appropriate to that purpose; and I am sorry that it must consequently still stand over, subject however to a determination to fulfil my promise on the earliest opportunity.

In the mean time, a few remarks on interesting mechanical subjects, may not prove unacceptable.

Hardening of steel without warping.

The present paper contains some practical experiments on hardening steel; the results of which have, in a great measure, proved successful in preventing *warping*: an inconvenience hitherto inseparable from the operation.

The usual process bends and spoils the work.

The process usually practised for hardening, is to heat the steel gradually to a red heat, and then plunge it into cold water, which produces the desired effect; but it is a subject of regret with all workers of this metal, that the figure of their work, is frequently changed by the operation, to such a degree, as to render useless all previous labour, and accuracy of workmanship.

The subject does not require theoretical disquisitions.

The limited extent of human knowledge respecting the organization of matter, will only allow us to speak hypothetically as to the occult causes to which these effects are referable. I shall not, therefore, on the present occasion, cloud the investigation of familiar operations, with the subtilties of philosophical disquisition; but proceed to the more useful part of my task.

Heated steel contracts, by cooling, to its first dimensions.

Pyrometrical experiments prove that steel, when heated so as to carry expansion to its utmost limit, if suffered to cool gradually and of its own accord, will return precisely to its original figure and dimensions. The detrimental effects produced by the operation of hardening, must therefore be occasioned, by some derangement of the particles, on the sudden expulsion of caloric.

Inference.

Keeping this idea in my mind, I thought, perhaps, if a piece of steel were repeatedly heated to different degrees below the hardening point, and as frequently quenched in cold water, this process might operate alteratively; and induce a different arrangement, more favourable to the instantaneous expulsion, of a larger proportion of caloric.

To prove this I made experiments with three cylindrical pieces of steel, six inches long, and half an inch diameter, accurately turned: the first of which I hardened in the usual way, and on examination, found it had deviated from a straight line ·05 In. The second piece, I heated just sufficient to occasion a faint hissing noise when dipped in the water; then a second time a little hotter, and quenched it as before: repeating this operation four or five times, increasing the degree of heat each time; keeping, however, below the hardening point till the last, where it was heated to a blood red and hardened; and [illegible] my surprise remained as perfectly straight and unaltered, as before the operation.

Experiments. A piece of steel fully heated and suddenly cooled warped. another piece repeatedly heated and cooled at successively increased heats did not.

The third piece I treated in the same way, and experienced nearly similar results; and since the time of making these experiments, I have had various opportunities of practising the process; and in every instance have found it effectual beyond my expectation.

A third piece was treated like the second, and with the same result.

For smaller articles, to which the above method is not applicable, I have found that by using water whose temperature is raised to 200°, the steel is not only perfectly hardened, but preserved from the disagreeable consequence, which the use of water at its common temperature, in general produces.

Small articles were hardened in hot water.

In the hope that the results of these experiments may prove useful, I offer them for publicity through the medium of your valuable journal; from which I readily acknowledge to have derived many hints myself, which have proved important in practice as well as theory.

E. LYDIATT.

London, Dec. 5th, 1812.

Annotation. W. N.

As the hardening of steel is higher, and the contraction by cooling, less, the greater the heat of the ignition. I many years ago, endeavoured to equalize the heat by igniting in a bath of red hot lead, (See Philos. Journal, quarto, No. 129.) which I have constantly found to answer. This method is particularly applicable

Another method by heating in melted lead.

cable to broad flat articles or such as have thick and thin parts. Perhaps the combination of both methods may in various cases be found useful.

VII.

Chemical Observations on the Sepia of the Cuttle Fish. By Mr. Grover Kemp. *Received from the Author.*

Introduction.

THE sepia of the cuttle fish having seldom attracted the notice of chemical writers when treating on animal substances, and its nature and properties being consequently but imperfectly known, the following experiments, it is presumed, will not be unacceptable to the public; since, without being conclusive, they may throw some light on the subject, and open the way to further investigation.

Description of the cuttle fish.

The cuttle fish, called by Icthyologists the sepia, or ink fish, is a genus of vermes mollusca. Its body is often nine inches in length, and three and a half in breadth; the head being attached to it somewhat in the same way as in the tortoise. It has ten tentacula, two of which are longer than the rest, and pedunculated. Its mouth is furnished with a strong beak of a horn colour, the upper mandible of which is hooked like the bill of birds of the falcon tribe. Its back is formed by a peculiar white pithy substance of a friable texture and oblong shape.

Os Sepiæ, or cuttle fish bone.

This is the well known Os Sepiæ, or cuttle fish bone of commerce, which is used for taking off the impressions of seals and medals; forming also a common ingredient in dentifrice. It is exactly similar in composition, according to Hatchett, to mother-of-pearl shells, 100 parts consisting of about 24 parts membrane, and 66 carbonate of lime. This bone has no flesh on it, but is merely covered by the external membrane or skin of the fish. A very singular property of this fish is the power which it possesses of emitting voluntarily a black liquor, not out of its mouth, as some naturalists assert, but from a small opening at the upper part of the belly, which communicates by a narrow duct, to a bag or bladder, situated near the coecum; in which this liquor is formed. The cuttle fish is said to avail itself of this property when chased by other fishes, and thus, rendering the water turbid and opake, it is enabled to elude their

Singular property of emitting a black liquor,

by which the fish is said to avoid its enemies.

their search.—Now it is to an examination of this peculiar liquor or sepia, supposed by Rondelet to be the bile of the cuttle fish, that I beg leave to call the attention of the reader; premising, however, that the sepia which I used was taken immediately from the fish, as no dependence can be placed on that which is exposed for sale, which is probably mixed with gum arabic, or some other foreign ingredient.

The sepia, when fresh, is a black glary liquid of a viscid consistence, a peculiar fishy smell and very little taste.

Properties of sepia.

Being subjected to experiment it afforded the following results.

1. It mixed readily with distilled water in any proportion, and shewed little or no disposition to subside after standing many hours: when the mixture was submitted to filtration, a considerable quantity of sepia was left behind, and what passed the filter was a thin black liquid, being a saturated solution of sepia in water.

It mixes with water.

2. Being poured into alcohol it coagulated immediately.

3. The same effect was produced by mixing it with ether.

Coagulates with alcohol and with ether.

4. Alcalies appeared to facilitate the solution of sepia in aqueous menstruum; Potash changing its colour to a brown, but ammonia not affecting it, after, however, it had undergone spontaneous evaporation to dryness, it became sparingly soluble in solutions of pure fixed and volatile alcali, but its colour remained unaltered by either.

Alcalies assist its solution.

5. When some of the saturated solution of sepia was boiled the sepia coagulated.

6. But when a very weak solution of it was boiled, coagulation did not take place.

It coagulates by boiling if saturated, but not if weak.

7. The sepia which was precipitated from its solution by boiling, was soluble in nitric acid when assisted by heat.

The last coagulation was sol. in hot nit. a.

8. After separating by filtration the sepia coagulated by boiling, from the water in which it had been dissolved, a precipitate was obtained by dropping in tinct. of galls.

The clear liquor of No. 5, was precip. by galls,

9. A light brown precipitate was also obtained by adding a solution of oxymuriate of mercury to another quantity of the water.

and also by ox. mur. acid.

10. The sulphuric, nitric, and muriatic acids precipitated the sepia from its solution in water. The sulphuric and muriatic

Sol of sepia is precip. by acids,

did not affect its colour, but the nitric after standing a day or two changed it to a brown.

but not by ox. m. acid.

11. Oxymuriatic acid did not occasion a precipitate with the solution of sepia; and mixed in the proportion of one part of the former to three of the latter, the colour was not affected; but when mixed in equal parts, it was changed to a brown.

Dried s. is insol. in ox. m. a.

12. Sepia, after having been dryed by spontaneous evaporation, was insoluble in oxymuriatic acid.

Ox. mur. of mercury precip. copiously,

13. A solution of oxymuriate of mercury being added to a solution of sepia, occasioned a copious precipitate.

as does nitrate of silver,

14. Nitrate of silver precipitated sepia from its solution in water, but did not injure its colour.

and also sulph. of iron.

15. Some solution of sulphate of iron, being dropped into a solution of sepia, the sepia was precipitated, but its colour was not affected.

Deductions.

From the above experiments, particularly from 2, 3, 5, 6, 7, 11, and 13, we may reasonably infer, that the sepia is composed for the most part of albumen. Example 8 and 9 indicate the presence of gelatine.

Sepia stands well as a colour.

As the oxymuriatic and nitric acids have so little effect on the colour of sepia, we may confidently conclude that it possesses the valuable property of standing well. This conclusion is also strengthened, and in a great measure confirmed, by the information of Dr. Leigh, from whom we learn that sepia has been sometimes used as writing ink, and that in a piece of writing of ten years standing, which he had seen, the colour of the sepia was still retained.

Indian ink does not appear to be sepia.

It has been conjectured by some writers,* that Indian ink is nothing else than the sepia of the cuttle fish. A very intelligent gentleman, with whom I corresponded on the subject, and who was of a contrary opinion, writes me as follows: "I have great reason to believe that not a particle of sepia enters into the composition of Indian ink. The colour is very different;

Sepia is far superior.

and sepia is as superior to Indian ink with respect to the ease of working, as Indian ink is to lamp black. I do not mean to say that it makes a clearer shadow; but Indian ink dries much quicker than sepia—an important consideration where a very large pale shadow is wanted. If too, a mistake be made with sepia, it may be washed almost clean off, whereas part of the Indian ink, if once dry, will adhere to the paper and resist every effort to remove it, without absolutely rubbing up the surface. I could point out other differences between Indian ink and sepia."

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* "Sepia piscis est qui habet succum nigerrimum instar atramenti quem inenses cum brodio oriza vel alterius leguminis inspissant et formant, et universum orbem transmittunt, sub nomine atramenti Chinensis."—li Hermani cynosura, t. 1. p. 17, pars II. Vide etiam Elemens de par M. Chaptal, tom. iii. p. 357, Montpellier edit. 1790.

To such artists as, by residing near the coast, have an opportunity of procuring the cuttle fish from fishermen, I would recommend the following simple means of preserving the sepia.—After carefully taking the bag out of the fish, having previously secured the duct by a ligature to prevent the sepia from running out, empty the contents of it into a saucer or gallipot, and after spreading it round the sides of the vessel, suffer it to dry gradually by exposure to the air. The reason for only coating the sides of the vessel is in order that it may dry before putrefaction commences.

Sepia dried gradually by exposure to the air may be kept for any time.

In this dry state it will keep for any length of time, and will always be fit for use, by being rubbed up with a little water.

GROVER KEMP.

Brighton, 11 *Mo.* 26, 1812.

VIII.

On the Motions of the Tendrils of Plants. By THOMAS ANDREW KNIGHT, *Esq. F. R. S. From the Philosophical Transactions for* 1812.

THE motions of the tendrils of plants, and the efforts they apparently make to approach and attach themselves to contiguous objects, have been supposed by many naturalists to originate in some degrees of sensation and perception: and though other naturalists have rejected this hypothesis, few, or no experiments have been made by them to ascertain with what propriety the various motions of tendrils, of different kinds, can be attributed to peculiarity of organization, and the operation of external causes. I was consequently induced, during the last summer, to employ a considerable portion of time

The tend plants have been supposed to move from sensation and perception.

time to watch the motions of the tendrils of different species of plants; and I have now the pleasure to address to you an account of the observations I was enabled to make.

Experiments with the Virginia creeper, the ivy, the common vine, and the pea.

The plants selected were the Virginia creeper (the ampelopsis quinquefolia of Michaux,) the ivy, and the common vine and pea.

The V. creeper insulated, turned its tendril towards a wall eight feet distant.

A plant of the ampelopsis, which grew in a garden pot, was removed to a forcing house in the end of May, and a single shoot from it was made to grow perpendicularly upwards, by being supported in that position by a very slender bar of wood, to which it was bound. The plant was placed in the middle of the house, and was fully exposed to the sun; and every object around it was removed far beyond the reach of its tendrils. Thus circumstanced, its tendrils, as soon as they were nearly full grown, all pointed towards the north, or back wall, which was distant about eight feet: but not meeting with any thing in that direction to which they could attach themselves, they declined gradually towards the ground, and ultimately attached themselves to the stem beneath, and the slender bar of wood.

Another plant differently situated, turned its tendril to the part most shaded,

and when fully illuminated, turned to an opaque object;

but not to a transparent one,

A plant of the same species was placed at the east end of the house, near the glass, and was, in some measure, screened from the perpendicular light; when its tendrils pointed towards the west, or centre of the house, as those under the preceding circumstances had pointed towards the north and back wall. This plant was removed to the west end of the house, and exposed to the evening sun, being skreened, as in the preceding case, from the perpendicular light; and its tendrils, within a few hours, changed their direction, and again pointed to the centre of the house, which was partially covered with vines. This plant was then removed to the centre of the house, and fully exposed to the perpendicular light, and to the sun; and a piece of dark-coloured paper was placed upon one side of it just within the reach of its tendrils; and to this substance they soon appeared to be strongly attracted. The paper was then placed upon the opposite side, under similar circumstances, and there it was soon followed by the tendrils. It was then removed, and a piece of plate glass was substituted; but to this substance the tendrils did not indicate any disposition to approach. The position of the glass was then changed, and care was taken to adjust its surface to the varying position of the sun, so that the light reflected might continue to strike the tendrils; which then receded from the glass, and appeared to be strongly repulsed by it.

and receded from a radiating one.

The claws of the vine were similarly affected, but at less distances.

The tendrils of the ampelopsis very closely resemble those of the vine, in their internal organization, and in originating from the alburnous substance of the plant; and in being, under certain circumstances, convertible into fruit stalks. The claws, or claspers of the ivy, to experiments upon which I shall now proceed, appear to be cortical protrusions only; but to be capable, I have reason to believe, of becoming perfect roots, under favourable circumstances. Experiments, in every respect very nearly similar to the preceding, were made upon this plant; but I found it necessary to place the different substances, to which I proposed that the claws should attempt to attach themselves, almost in contact with the stems of the plants. I observed, that the claws of this plant evaded the light, just as the tendrils of the ampelopsis had done; and that they sprang only from such parts of the stems as were fully or partially shaded.

The stems of the V creeper, and the ivy, inclined towards a tree.

A seedling plant of the peach tree, and one of the ampelopsis and ivy, were placed nearly in the centre of the house, and under similar circumstances; except that supports, formed of very slender bars of wood, about four inches high, were applied to the ampelopsis and ivy. The peach tree continued to grow nearly perpendicularly, with a slight inclination towards the front and south side of the house, whilst the stems of the ampelopsis and ivy, as soon as they exceeded the height of their supports, inclined many points from the perpendicular line, in the opposite direction.

Not only the tendrils, but the stems of plants, incline to their supports.

It appears, therefore, that not only the tendrils and claws of these creeping dependent plants, but that their stems also, are made to recede from light, and to press against the opaque bodies, which nature intended to support and protect them.

This effect is opposite to the inclination of succulent plants.

M. Decandole, I believe, first observed, that the succulent shoots of trees and herbaceous plants, which do not depend upon others for support, are bent towards the point from which they receive light, by the contraction of the cellular substance of their bark, upon that side, and I believe his opinion to be perfectly well founded. The operation of light upon the tendrils and stems of the ampelopsis and ivy, appears to produce diametrically opposite effects, and to occasion an extension of the cellular bark, wherever that is exposed to its influence; and this circumstance affords, I think, a satisfactory explanation why these plants appear to seek and approach contiguous opaque objects, just as they would do, if they were conscious of their own feebleness, and of power in the objects to which they approach, to afford them support and protection.

The vine confirms the same explanation.

The tendril of the vine, as I have already stated, is internally similar to that of the ampelopsis, though its external form, and mode of attaching itself, by twining round any slender body, are very different. Some young plants of this species, which had been raised in pots in the preceding year, and had been headed down to a single bud, were placed in a forcing-house, with the plants I have already mentioned; and the shoots from these were bound to slender bars of wood, and trained perpendicularly upwards. Their tendrils, like those of the ampelopsis, when first emitted, pointed upwards; but they gradually formed an increasing angle with the stems, and ultimately pointed perpendicularly downwards; no object having presented itself to which they could attach themselves.

Other plants of the vine.

Other plants of the vine, under similar circumstances, were trained horizontally; when their tendrils gradually descended beneath their stems, with which they ultimately stood very nearly at right angles.

A third set of plants were trained almost perpendicularly downwards; but with an inclination of a few degrees towards the north; and the tendrils of these permanently retained very nearly their first position, relatively to their stems; whence it appears, that these organs, like the tendrils of the ampelopsis, and the claws of the ivy, are to a great extent under the control of light.

The vine differs from the creeper.

A few other plants of the same species were trained in each of the preceding methods; but proper objects were placed, in different situations, near them, with which their tendrils might come into contact; and I was by these means afforded an opportunity of observing, with accuracy, the difference between the motions of these and those of the ampelopsis, under similar circumstances. The latter almost immediately receded from light, by whatever means that was made to operate upon them;

and they did not subsequently shew any disposition to approach the points from which they once receded. The tendrils of the vine, on the contrary, varied their positions in every period of the day, and after returned again during the night, to the situations they had occupied in the preceding morning; and they did not so immediately, or so regularly, bend towards the shade of contiguous objects. But as the tendrils of this plant, like those of the ampelopsis, spring alternately from each side of the stem, and as one point only in three is without a tendril, and as each tendril separates into two divisions, they do not often fail to come into contact with any object within their reach; and the effects of contact upon the tendril are almost immediately visible. It is made to bend towards the body it touches, and if that body be slender, to attach itself firmly, by twining round it, in obedience to causes which I shall endeavour to point out.

Explanation of the causes why the tendril of the vine assumes its motions and curvature.

The tendril of the vine, in its internal organization, is apparently similar to the young succulent shoot, and leaf-stalk, of the same plant; and it is as abundantly provided with vessels or passages for the sap; and I have proved, that it is alike capable of feeding a succulent shoot, or a leaf, when grafted upon it. It appears, therefore, I conceive, not improbable, that a considerable quantity of the moving fluid of the plant passes through its tendrils; and that there is a close connection between its vascular structure and its motions.

I have proved, in the Philosophical Transactions of 1806, that centrifugal force, by operating upon the elongating plumules of germinating seeds, occasions an increased growth and extension upon the external sides of the young stems, and that gravitation produces correspondent effects; probably by occasioning the presence of a larger portion of the fluid organizable matter of the plant upon the one side, than upon the other. The external pressure of any body upon one side of a tendril, will probably drive this fluid from one side of the tendril, which will consequently contract to the opposite side, which will expand; and the tendril will thence be compelled to bend round a slender bar of wood or metal, just as the stems of germinating seeds are made to bend upwards, and to raise the cotyledons out of the ground; and in support of this conclusion I shall observe, that the sides of the tendrils where in contact with the substance they embraced, were compressed and flattened.

The tendrils of the pea are affected as those of the vine.

The actions of the tendrils of the pea were so perfectly similar to those of the vine, when they came into contact with any body, that I need not trouble you with the observations I made upon that plant. An increased extension of the cellular substance of the bark upon one side of the tendrils, and a correspondent contraction upon the opposite side, occasioned by the operation of light, or the partial pressure of a body in contact, appeared in every case which has come under my observation, the obvious cause of the motions of tendrils; and therefore, in conformity with the conclusions I drew in my last memoir, respecting the growth of roots, I shall venture to infer, that they are the result of pure necessity only, uninfluenced by any degrees of sensation, or intellectual powers.

T. A. KNIGHT.

Downton, April 27, 1812.

IX.

Additional Experiments on the Muriatic and Oxymuriatic Acids.

By WILLIAM HENRY, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester. From the Philosophical Transactions,* 1812.

Introductory remarks.

THE experiments, which form the subject of the following pages, are intended as supplementary to a more extensive series, which the Royal Society did me the honour to insert in their Transactions for the year 1800[*]. Of the general accuracy of those experiments, I have since had no reason to doubt; and their results, indeed, are coincident with those of subsequent writers of the highest authority in chemistry. My attention has been again drawn to the subject by the important controversy which has lately been carried on between Mr. Murray and Mr. John Davy, respecting the nature of muriatic and oxymuriatic acids[†]; and I have been induced, by some hints which the discussion has suggested, not only to

Reference to the controversy between Mr. Murray and Mr. John Davy.

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[*] Page 188.

[†] Nicholson's Journal, XXVIII, and XXIX.

repeat the principal experiments described in my memoir, but to institute others, with the advantage of a more perfect apparatus than I then possessed, and of greater experience in the management of these delicate processes.

Uncertainty of an experiment of the quantity of hydrog. evolved from mur. ac. gas dried and not dried.

This repetition of my former labours has discovered to me an instance in which I have failed in drawing the proper conclusion from facts. In two comparative experiments on the electrization of equal quantities of muriatic acid gas, the one of which was dried by muriate of lime, and the other was in its natural state, I found a difference of not more than one *per cent.* in the hydrogen evolved, relatively to the original bulk of the gas*. Yet, notwithstanding these results, I have expressed myself inclined to believe, that some water is abstracted by that deliquescent salt; and this belief was confirmed, several years afterwards, by the event of an experiment in which muriatic acid gas, dried by muriate of lime, gave only $\frac{1}{35}$ its bulk of hydrogen†, a proportion much below the usual average. The question, however, was too interesting to be left in any degree of uncertainty; and I have, therefore, made several fresh experiments with a view to its decision. In the course of these I have found, that though differences in the results are produced by causes apparently trivial, some of which I shall afterwards point out, yet that under equal circumstances, precisely the same relative proportion of hydrogen gas is obtained from muriatic acid gas, whether exposed or not to muriate of lime; and that its greatest amount does not exceed $\frac{1}{16}$ or $\frac{1}{14}$, the original volume of the acid gas.

The quantity is the same, whether the muriatic acid gas be exposed or not to muriate of lime.

Muriate of lime does not unless humid, evolve heat with muriatic acid gas.

In the paper last quoted‡, I have also described an experiment, in which sensible heat was evolved by bringing muriate of lime into contact with muriatic acid gas; a fact which, if established, would go far to prove the existence of water in that gas. But on repeating the experiment with muriate of lime recently cooled from fusion, and over mercury carefully deprived of all moisture by boiling, I was not able to discover any increase of temperature, though a very sensible air thermometer was inclosed in the vessel containing the gas. The evolution of heat takes place, only when the muriate of lime has attracted moisture, either from the atmosphere or the mercury, and is then owing to a condensation of a part of the gas.

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* Page 191.

† Phil. Trans. 1809, page 433.

‡ Page 433, note.

Muriatic acid gas. over mercury when electrified, affords calomel and hydrogen; but to a certain extent only.

Essentially, the changes produced by electrifying muriatic acid over mercury are those which I have stated; viz. a contraction of the volume of the gas, the formation of muriate of mercury (calomel,) and the evolution of hydrogen. Recent experiments, also, have confirmed the accuracy of the observation*, that when a certain effect has been produced by electricity, nothing is gained by continuing the process; for neither is more hydrogen evolved, nor can the contraction of bulk be carried any farther.

Muriatic acid gas, electrified in a vessel, without the presence of any other fluid,

showed no moisture;

but when the muriatic acid gas was abstracted, the small residue was oxymuriatic acid gas and hydrogen.

I have lately applied, to experiments on muriatic acid, an apparatus which I used advantageously for the analysis of ammonia†. It consists of a spherical glass vessel, into which are hermetically sealed two small tubes containing platina wires, the points of which approach within the striking distance. To the globular part is attached a neck, which may be closed, as occasion requires, either by a glass stopper, or by a metal cap and stop-cock. Into a vessel of this kind I introduced $4\frac{1}{2}$ cubic inches of muriatic acid gas, and passed through it 3000 discharges from a Leyden jar; at the close of the process, no traces of moisture could be perceived on the inner surface of the vessel; nor could I discover, on opening the stopper, that any change of bulk had taken place. After absorbing the unchanged muriatic acid gas by a small quantity of water, a volume of gas remained, in which there were present 100 measures (each equal to one grain of mercury) of oxymuriatic acid gas, and 140 measures of hydrogen. Two causes might, perhaps, contribute to diminish, in some degree, the proportion of the former. It was difficult to exclude from the apparatus, on admitting the muriatic acid gas into it, two or three very minute globules of mercury, which became tarnished during the experiment, exactly as they would have been by oxymuriatic acid; and a small portion of the latter gas was probably also taken up by the water employed to absorb the muriatic acid.

Repetition, on

With the intention of giving greater effect to the electricity,

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* Phil. Trans. 1800, p. 192.

† Ibid. 1809.

I repeated the experiment in a vessel capable of containing not more than 1400 grains of quicksilver, (about ·41 of a cubic inch,) the neck of which being only one-fifth of an inch in diameter, was better calculated to show any minute change in the volume of the gas. On removing the stopper, however, no change of volume was apparent. The hydrogen evolved, instead of being more than in the former experiment, equalled in bulk only 20 grains of mercury. The production of oxymuriatic acid was sufficiently evinced by its effect in tarnishing some very small globules of quicksilver, which adhered to the inside of the vessel; but the minuteness of the quantity frustrated an attempt to measure it. From subsequent experiments on similar quantities of gas, confined in the same apparatus, it appeared, that the electrization in this last instance, had been continued much longer than was necessary; and that an equal effect was produced by one-eighth the number of electrical discharges.

a smaller scale, with the same result.

In this way of making the experiment, the greatest proportion of hydrogen gas obtainable from muriatic acid, amounted only to about $\frac{1}{70}$th, while, by electrization over quicksilver, $\frac{1}{16}$ or $\frac{1}{14}$ was generally evolved. It was evident, then, that the mercury had considerable influence over the results; and I found, by experiments with tubes of different diameters, that the larger the surface of the mercury exposed to the gas, the more rapid and complete was the change. Its action was greatly accelerated, also, by causing the electric discharge to strike from the conducting wire, sealed into the tube, to the mercury, which was probably thus raised into vapour; for in some instances, the whole of the inner surface of the glass was coated with sublimed calomel.

The hydrogen evolved when mercury is not present, is 1-70th; but if present, 1-15th, or nearly five times as much.

The only way in which the mercury appeared to me likely to be efficient in this case, was by removing the oxymuriatic acid as fast as it was formed; for I have never found any mixture of this gas in the results of experiments on muriatic acid, when carried on over quicksilver. Upon any theory of the constitution of muriatic acid, it may be expected that when, in a mixture of that acid gas with hydrogen and oxymuriatic acid gasses, the two latter come to bear a certain proportion to the former, they will be brought within the sphere of mutual agency, and will reproduce muriatic acid. This point appears,

The mercury appears to act by removing the oxymuriatic acid as formed; and preventing the reproduction of common muriatic acid, when the oxymuriatic acid became abundant;

from my experiments, to be attained, when the hydrogen and oxymuriatic acid, taken together, have the proportion to the muriatic acid, of about 1 to 35. The amount of the change, therefore, which is capable of being effected on muriatic acid gas, electrified without the contact of mercury, is limited by the reaction of the evolved hydrogen and oxymuriatic acid gasses on each other, whenever they compose a certain proportion of the mixture. This proportion being attained, we only, by continuing the electrization, work in a circle.

viz. both gases to the muriatic as 1 : 35.

Muriatic acid over mercury,

It may now be inquired, what is the limitation to the action of electricity on muriatic acid gas, which is confined over mercury? In this case it was suggested to me by Mr. Dalton, who favoured me with his presence at most of the experiments, that the evolved hydrogen might possibly in some way prevent the effect from being carried beyond a certain amount. Availing myself of this hint, I mixed thirty measures of hydrogen gas with 400 of muriatic acid gas in its ordinary state, and passed 900 discharges through the mixture. It soon became evident that the addition of the hydrogen had produced an important difference in the results of the experiments; for the surface of the mercury, over which the gas rested, was untarnished after some hundred explosions, and was scarcely changed at the close of the process. When the residuary gas, the volume of which remained unaltered, was analyzed, it was found to contain the same quantity of muriatic gas, as at the outset, and neither more nor less hydrogen.

with about 1-13th its bulk of hydrogen, is not changed by electrization,

To explain the event of this modification of the experiment, on the old theory, we may suppose, that, by the action of electricity, a particle of water is decomposed, and that the atom of oxygen, forcibly repelled from that of hydrogen with which it was associated, finds another atom of hydrogen uninfluenced by the electric fluid, and within the sphere of its attraction. With this it unites, and recomposes water. On the theory of Sir H. Davy, the same series of decompositions and recombinations may be assumed to take place between the oxymuriatic acid and hydrogen*. It

because the decomposed water finds hydrogen to recompose it:

or the muriatic acid is decomposed and recomposed.

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* I am aware, that there is an apparent inconsistency in supposing changes of precisely an opposite kind to be effected by the same means. But instances are not wanting, in which the very same elements are brought into combination by electric discharges, and are again disunited by

Qu: Whence comes the limitedhydrog. evolved by electricity? —from water as an element in the gas? —or from the disunion of hydrog. from oxymur. acid as elements of mur. acid?

It still, however, remains to be determined, what is the source of the hydrogen gas, which, in a limited proportion, is always evolved by the electrization of muriatic acid? Does it result from the decomposition of water, existing as an element of the gas; or from the disunion of the oxymuriatic acid and hydrogen, which, according to Sir H. Davy's view, compose muriatic acid? The limitation to its amount, which, it formerly appeared to me*, could only be accounted for by the complete destruction of the water contained in the gas, may now be equally well explained, on the principle which I have just pointed out. The fact, also, that no appreciable change of bulk is produced by the electrization of the muriatic acid, when the presence of mercury is excluded, is perhaps favourable to the new theory. For since equal measures of hydrogen and oxymuriatic acids afford muriatic acid without any condensation of volume, no alteration of bulk should result from the disunion of those elements; and the products should be equal measures of the same gases. The proportions, which I obtained (100 to 140) did not, it must be acknowledged, exactly correspond with the theory; but the difference was not greater, than might naturally be expected from the circumstances of the experiment. That equal measures of hydrogen and oxymuriatic acid are really evolved, appears to me to be proved by the agreement, which I have in several experiments remarked, between the hydrogen gas obtained, and the contraction of volume in muriatic acid electrified over mercury. Now the latter effect of the process can be explained on no other principle than the absorption of oxymuriatic acid by the quicksilver.

Perhaps the latter; because the volumes are equal.

When muriatic acid and oxygen gases are electrified toge-

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by the same agency. As examples, it may be sufficient at present to state, that nitrous acid and nitrous gas are generated by the action of the electric spark on mixtures of oxygen and nitrogen gases; and that, by the same power, they are again resolved into their elements. If this were the proper place, it might, I think, be rendered probable by several arguments, that electricity, when thus applied acts rather by mechanical collision, than by inducing a change in the electrical states of the elements of bodies.

* Phil. Trans. 1800, p. 200.

Muriatic acid and oxygen gases, electrified alone, afford *water* and oxymuriatic acid gas.

ther over mercury, a gradual diminution ensues in their bulk,* and the mercury becomes tarnished, precisely as by the contact of oxymuriatic acid. I have lately examined the agency of this process on a considerable quantity of the two gases confined in a vessel, into which they were admitted after exhausting it by the air-pump. The phenomena, which in this way of making the experiment are extremely decisive and interesting, are the production of water and of oxymuriatic acid. The former, combining with a portion of the undecomposed muriatic acid, is deposited in drops upon the inner surface of the vessel, in the state of liquid muriatic acid. When the stop-cock, which confines the gases, is opened under mercury, a quantity of that metal rushes in, and has its surface instantly tarnished. Besides this test of the production of oxymuriatic acid, its presence is rendered unequivocal (after absorbing the undecomposed muriatic acid by a few drops of water), both by its smell, and by its effect in discharging the colour of litmus paper†.

These results agree with either theory. The oxigen may unite with the muriatic acid and water be *deposited*,— or the oxigen may unite with hydrogen as one of the principles of mur. acid and *form water*, *while the* oxymur. acid is disengaged as the other principle.

These results, it will be found, may be reconciled with either theory. According to the one which has been commonly received, the oxygen unites with the real acid of muriatic gas, which becoming oxymuriatic acid, *deposits* water. On Sir H. Davy's view, the oxygen unites with the hydrogen of the muriatic acid, and *composes* water, while the oxymuriatic acid is merely an educt. I am not aware of any refinement of the process, by which the value of these two explanations can be compared. Something, however, would be gained by a precise determination of the proportions, in which the two gases saturate each other. For since, on Sir H. Davy's theory, muriatic acid contains half its volume of hydrogen gas, two measures of which are known to be saturated by one of oxygen, it follows that muriatic acid gas should be changed into oxymuriatic by one-fourth of its bulk of oxygen. According to Gay Lussac and Thenard*, three measures of muriatic acid should condense one of oxygen (or only one-third their bulk), and should form two measures of oxymuriatic acid. Hitherto, I have not been able to satisfy myself respecting the true proportions of oxygen and muriatic acid gases, that are capable of being united by electricity; for though I have made several experiments with this view, they have not agreed in yielding similar results. The condensation of a part of the undecomposed acid by the water, which is formed during the process, will, probably, indeed, always be an impediment to our learning these proportions exactly. The fact is chiefly of value, as it affords an example of the production of oxymuriatic acid under the simplest possible circumstances; and as it shews unequivocally that, under such circumstances, the visible appearance of moisture is a part of the phenomena.

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* Phil. Trans. 1800, p. 193.

† Those who wish to repeat this experiment need not be deterred by the apprehension of the labour attending it; for 3 or 400 discharges, from a Leyden jar of moderate size, are sufficient to occasion a disti precipitation of moisture. When a mixture of oxygen and m acid gases is even suffered to stand over mercury, a gradual of volume takes place; the muriatic acid, if in proper proportion, entirely disappears; and calomel is deposited upon the surface of the glass vessel; but, in this case, there is no visible production of moisture.

It may be of value to theory to ascertain the proportions of oxygen and mur. acid gas which form oxym. acid. but this has not yet been effected.

Manchester, Jan. 6, 1812.

XI.

Experiments on Putrefaction. By JOHN MANNERS, *M. D. of Philadelphia. In a letter from the Author.*

To Mr. Nicholson.

SIR,

FROM reading a paper upon the *vinous* and *putrefactive* fermentation by Gay Lussac, in a late number of your "Philosophical Journal," in which the author, according to the general opinion of chemical philosophers, contended that the access of atmospheric air or oxygen gas, was a *sine qua non* of the process, I was induced to institute the following experiments on *putrefaction*, by which I have proved (as I conceive) beyond the possibility of exception, that oxygen is not only [illegible]ntial to the putrefactive fermentation, but has, when in actual contact with the putrefying substance, no influence on that process.

Whether oxygen be requisite to putrefaction.

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* Mémoires d'Arcueil, ii. 217.

Muscular flesh was included in air above distilled water.

I secured some fresh muscular flesh (a portion of lamb) in the bottom of a glass jar, and inverted it over distilled water, observing that the water within the jar was precisely on a level with that which was external; that any absorption of either of the components of the intercluded atmospheric air, might be noted by a corresponding absorption of water within the jar.

Fahrenheit's thermometer stood at 70°. At which temperature it was kept during the experiment.

The water contained no oxygen.

That the distilled water was perfectly free from any oxygen gas, I proved by Mr. Dusseu's method : viz. I tinged a portion of the same water with *litmus*, and passed *nitrous gas* through it, which Dr. Priestley proved would combine with the oxygen, and be converted into *nitric acid*, which would change the litmus *red*. Dr. Thompson says, however, that this is not a critical test, and that the litmus will not be changed unless there be an unusual quantity of oxygen gas present.

Remarks on eudiometry.

Upon the discovery of this property of nitrous gas, by Dr. Priestley, he founded the first eudiometer, which has been since improved by Falconer, Fontana, Cavendish, Ladriani, Magellan, Baron Von Humboldt, Engenhausz, Dalton, and Gay Lussac, and contributed so much to extend the bounds of philosophical knowledge. Before this important era, the only eudiometer in the hands of the philosophers, was a *sparrow*, a *mouse*, or a *taper*. Since, however, others have been devised; as the sulphuret of iron by Scheele, the liquid hydro-sulphuret of potash by De Marti, the rapid combustion of phosphorus by Humboldt and Seguin, the slow combustion of phosphorus by Berthollet, the green sulphate and muriate of iron impregnated with nitrous gas by Davy, and the detonation of hydrogen gas by Volta.

The putrefaction made no change in the included air

The jar remained three days, during which time the flesh had undergone the putrefactive process, as was evinced by the offensive odour emitted. But at no time could I observe any absorption of water within the jar: except where there was a corresponding reduction of atmospheric temperature, and in consequence a condensation of the intercluded air. But Dr. Priestley, in a similar experiment, found a small augmentation of air within the jar; as I have in subsequent experiments.

The confined air was analysed with the eudiometer of

Humboldt, but not found to differ from atmospheric air in the proportion of its oxygen and nitrogen.

The state of the *barometer*, however, was not attended to in this experiment, which renders it liable to exception. Neither did Dr. Priestley attend to the state of the barometer in his experiments, or if he did he omitted to mention it.

Repetition of the experiment over mercury; with no absorption.

I repeated this experiment over mercury. The thermometer as before stood at 70, and the barometer at 29·1 inches. The experiment was continued three days, when the putrefactive fermentation had taken place, as was evinced by the odour emitted: but there was at no time any absorption of mercury within the jar. Upon examining the included air with the eudiometer it was not found to differ from atmospheric air.

The same repeated with a different mercurial apparatus,

But to magnify and render more conspicuous any absorption, in consequence of a diminution of the included atmospheric air, by the combination of its oxygen with the animal matter, I invented an instrument which I shall now describe.

I took a cylindrical bottle perfectly transparent, and put half a pound of muscular flesh (a portion of the diaphragm of a bullock) in the bottom of it and secured it there. The flesh was taken while warm, and cooled under mercury to prevent the access of air. To the bottle was adapted a cork which was perforated, and a bent tube passed through the perforation, the other end of which was hermetically sealed. Some mercury was then put into the bottle—the bottle corked and made perfectly air-tight by luting and sealing—the bottle was now inverted. The mercury filled about two inches of the neck of the bottle, and was made to pass up the glass tube by heating it, and expanding the air, and thus expelling a portion of it to a proper distance. In this situation the bottle was put to rest in a fixed position. A thermometer was included within the bottle in order to note the temperature.

The bottle and curved tube in some measure represented Mr. Leslie's Differential thermometer. The barometrical influence was perfectly excluded. And as variations in the temperature equally affected both the air included in the tube, and that in the bottle, it is evident the thermometrical influence could not affect the experiment. To the tube was adapted a graduated scale, which would mark any rise or fall of mercury in the tube.

calculated to shew minute variations.

Now it is clear that the smallest diminution of air in the bottle would be marked by a corresponding fall of the mercury in the tube, the calibre of which was not more than one line. Or, on the contrary, any evolution of gas would raise the mercury in the tube.

No absorption took place.

The apparatus remained three days without any change of mercury in the tube. On the fourth day the mercury began to rise, and continued to rise until the experiment was suspended, which proves that there was no absorption of oxygen gas by the putrefying substance. The thermometer included in the bottle stood at 60, during the experiment. This apparatus is easily constructed and may be used for many similar purposes as a gasometer.

Putrefaction effected without access of air:

As from all these experiments it appeared that no oxygen gas was absorbed by the putrefying substance, I determined to exclude the atmospheric air altogether. This I attempted *first* by the following experiment.

In carbonic acid gas

I put some fresh meat into a glass vessel—filled it with mercury—placed it in the pneumatic cistern, and filled it with carbonic acid. In this situation it was kept three days; at the end of which period the flesh was found to have undergone the putrefactive fermentation; yet all air except carbonic acid was excluded. Though Sir John Pringle and Dr. Mc Bride (and the latter from actual experiments) contended for the antiseptic powers of fixed air. The thermometer during this experiment stood at 70.

and in hydrogen gas.

But as my object was to exclude oxygen gas, and as carbonic acid contains that as one of its components, I thought it not impossible but that the animal matter might abstract a portion of the oxygen from the carbonic acid, and convert it into carbonic oxyde: as is the case with *iron, zinc, tin,* and certain other metals. I therefore repeated the experiment in every circumstance as before, except that I substituted *hydrogen* for *carbonic acid* gas: but with precisely the same result, putrefaction went on as well as in any of my former experiments.

and in other gases.

I tried *sulphuretted hydrogen* and *nitrogen* gases in the same manner, and with the same result.

I then fell upon a *second* method of excluding atmospheric air and oxygen gas.

I to

Repetition of the experiments with the flesh closely surrounded or immersed in mercury.

I took an eight ounce phial and put six ounces of fresh beef (a portion of the diaphram) in the bottom of it, and secured it there. In procuring this beef I was so careful as to go to the butcher's myself, and have it cut off the moment the animal was dead. Upon this meat while thus warm and not affected by external air, I placed a column of mercury, by filling the phial with that fluid. The phial was corked, and to the cork was adapted one leg of a syphon, which perforated the cork, all which was made perfectly air tight by luting and sealing.

The syphon was filled with mercury completely, and passed into the mercurial cistern. Over this was placed a glass vessel filled with mercury and inverted, in order to collect any gas that should come over.

That there was now a complete column of mercury from the meat to the top of the vessel inverted in the cistern. My object in the *first* place, was to prove by the first phial containing the meat covered with a column of mercury, whether putrefaction could take place in that situation where the possible access of air was cut off by the mercury. My object with the syphon and other apparatus was to collect and to examine the products, if putrefaction should proceed: the thermometer stood during this experiment at 70°.

In about three days the putrefactive process was evidently going on.

Conclusion. That oxygen is neither essential to, nor has any influence in, putrefaction:

These experiments were sufficient to satisfy me, that atmospheric air or oxygen gas, is so far from being essential to putrefaction, that it has no influence on that process where it has free access to the putrefying substance. These experiments have since been repeated and confirmed by my friend Dr. Mitchill at my request.

but that it is caused by changes in the substance itself.

Therefore I am disposed to believe, that putrefaction must depend on the destruction of the equilibrium of attractions, which in the living state of animals exists among the *elementary principles* of which they are composed, by the loss of *vitality*: by which new attractions are called into action, and new *combinations* and *decompositions* take place.

Products of putrefaction.

My next object was to examine the products of putrefaction which had taken place without *extrinsic* oxygen.

Serum.

The *first* product was a *bloody serum*.

Gas.

The second was a transparent gas, possessing the *transparency*, *elasticity, dilatibility, compressibility,* and other mechanical properties of atmospheric air.

As the gaseous products of putrefaction had never been collected and chemically examined, I thought it an object of importance to give it a *careful* and *critical* analysis.

Examination of the gas from putrefaction;

I therefore proceeded to examine it in the following manner.

As it has been the unanimous opinion of chemical philosophers, who have written upon the subject, that *ammonia* is generated, and is the principal product of putrefaction, I first tested for that substance.

by litmus and acid; no change.

1st. By passing a piece of *litmus paper, reddened by vinegar,* into a vessel about half filled with this gas over mercury—no change.

2d. Some of the gas was passed through an *infusion of litmus* reddened by vinegar—no change.

3d. I filled a vessel with mercury over the mercurial bath, and displaced about half of it by passing up an infusion of *litmus reddened by vinegar.* After which I passed up the gas, which was somewhat *absorbable,* until it was strongly impregnated with it, and had accumulated in the top of the vessel—no change.

by turmeric; no change.

4th. 5th. and 6th. I tested it with *turmeric* in all the three ways in which litmus reddened by vinegar was used—no change.

The gas was not absorbed by water.

7th. I passed up a piece of *wet sponge* by means of a wire, but there was no *perceptible* absorption of the gas by the water contained in the sponge.

8th. The sponge was withdrawn and washed in a solution of sulphate of copper—no change.

by sulphate of copper; no change.

9th. I passed up a solution of sulphate of copper into a vessel filled with mercury over the mercurial cistern until it was two thirds displaced by the solution of copper. I then passed up the gas until the solution was strongly impregnated with it and it had accumulated in the top of the vessel—no change.

by carbonic acid gas; no change.

10th. *Carbonic acid gas* was passed up into a vessel containing this gas. No chemical change (except with Mr. Berthollet we call the admixture of gases chemical *dissolution.*) The carbonic acid produced an augmentation in the bulk of the gases proportional to its quantity.

nor by muriatic acid gas.

11th. *Muriatic acid gas* was passed up into a vessel filled with gas over mercury—no change.

These experiments were abundantly sufficient to prove that there was no *ammonia* in the products of putrefaction, at least where it takes place without the influence of external causes.

Tried by phosphorus; no combustion.

Secondly, I tested it for oxygen in the following manner.

1st. A piece of *phosphorus* was passed up into a vessel filled with this gas and standing over mercury. The phosphorus was fused and became perfectly fluid, floating upon the surface of the mercury, by pouring boiling water upon the vessel. But there was not the *slightest appearance of combustion.*

2d. *Water* was now passed into the same vessel, which was tested *for phosphoric acid by litmus*—no change.

by nitrous gas; no diminution.

3d. *Nitrous gas* was passed into a vessel filled with this gas over mercury—no change, except in the bulk proportional to the gas added.

4th. Water was now passed up into the same vessel and tested for *nitric acid* by litmus—no change.

by a mouse.

5th. A mouse was passed into a vessel of this gas which instantly died.

Conclusion; no oxygen present.

These experiments were deemed sufficient to prove the *nonexistence* of *oxygen.*

Thirdly, It was now tested for *sulphuretted hydrogen.*

Silver shewed no sulph. hydro.

1st. A piece of *silver* was placed in a vessel of this gas which was *not blackened* or converted into a sulphuret when withdrawn. A piece of silver was also kept in water highly impregnated with this gas, and one was placed at the end of the tube from which the gas was disengaged, and with the *same result.*

Nitr. of sil. no change.

2d. The gas was passed through a solution of *nitrate* of *silver* —no change.

3d. A vessel was filled with mercury over the mercurial cistern, and displaced by passing up a *solution* of nitrate of *silver* until the vessel was half filled with it. After which the gas was permitted to pass up, as it was disengaged from the putrefying substance through the mercury, and through the solution of silver, until it was strongly impregnated with it and had accumulated in the top of the vessel—no change.

Acet. of lead no change.

4th. A solution of acetate of lead in the same manner as experiment 2d—no change.

5th. A solution of acetate of lead was impregnated with this gas in the same manner that nitrate of silver was in experiment 3 --no change.

Conclusion. No sulph. hydrog. was formed.

These tests were sufficient to prove that no sulphuretted hydrogen was formed.

Fourthly, I tested it for *carbonic acid.*

By litmus.

1st. 2d. and 3d. By *litmus* in all *three* of the ways in which litmus reddened by vinegar was used for ammonia—reddened.

and by lime water

4th. I filled a vessel with mercury over the mercurial bath, and passed up a quantity of lime water, after which I passed up the putrescent gas.—The *lime* was *precipitated.*

and then an acid.

5th. The precipitate effervesced with the *oxalic, sulphuric, nitric,* and *muriatic acids.*

The gas proved to be carbonic.

Consequently the gas is carbonic acid: Probably holding a fœtid oil (or some of the animal matter) in solution to which it owes its offensive odour.

From the six ounces of animal substance I have already collected 100 cubic inches of this gas.

I am, dear Sir,
Your obedient servant,
JOHN MANNERS.

Philadelphia, Oct. 12th, 1812.

XII.

Of the excellent Qualities of Coffee, and the art of making it in the highest perfection. By Benjamin, Count of Rumford, *F. R. S. Abridged from his 18th Essay, published in London,*

Praises of coffee.

THE Count begins his Essay with an eulogium on coffee. He celebrates it as uncommonly agreeable in its taste, salubrious in its effects, and producing exhilaration which lasts many hours, and is not followed by sadness, languor, or debility. The glow of health, the consciousness of increased vigour of mind it affords, and the uniform experience of many able, brilliant, and indefatigable men of the first talents in its favour—are among the topics on which the animated writer dwells in his praises of this most delightful vegetable. He acknowledges his own obligation to its powers, and society will admit that a more cogent instance could scarcely have been adduced in support of his argument.

The goodness of coffee depends greatly on its preparation :

But there is no culinary process so uncertain in its results as that of making coffee. The same materials, in the same proportions, shall produce good or bad coffee according to the management. If the peculiar aromatic flavour of coffee be dissipated and lost, its exhilarating quality is gone, and all that would have made it valuable. To prepare it as it ought to be done, is the object of the Essay before us.

Particularly the roasting.

Great care must be taken not to roast coffee too much. As soon as it has acquired a deep cinnamon colour, it should be taken from the fire and cooled : otherwise much of its aromatic flavour will be dissipated, and its taste will become disagreeably bitter.

This is best performed in a glass vessel.

In some parts of Italy, coffee is roasted in a thin Florence flask, slightly closed by a loose cork, and held over clear burning coals with continual agitation. No vapour issues from the coffee sufficient to prevent the progress of its roasting from being clearly seen. The Count has adopted this process by using a thin globular vessel of glass, with a long neck, which he closes, when charged, with a long cork, having a small slit on one side, to allow the escape of the vapours, and projecting far enough out of the neck to be used as a handle to turn the vessel round, while exposed to the heat of a chafing dish of coals. This vessel is laid horizontally, and is supported by its neck so as to be easily turned round ; which may be done without the least danger, however near the coals, provided the glass be thin, and kept constantly turned.

Instructions for roasting coffee.

In order that the coffee may be perfectly good, and very high flavoured, not more than half a pound of the grain should be roasted at once ; for when the quantity is greater, it becomes impossible to regulate the heat so as to be quite certain of a good result. The progress of the operation, and the moment most proper to put an end to it may be judged and determined with great certainty ; not only by the changes which take place in the colour of the grain ; but also by the peculiar fragrance which will first begin to be diffused by it when it is nearly roasted enough.

Coffee is best, if made immediately after roasting.

The roasted coffee must be carefully preserved.

If coffee in powder be not defended from the air, it soon loses its flavour and becomes of little value; and the liquor is never in such high perfection as when the coffee is made immediately after the grain is roasted. This fact is well known to those who are accustomed to coffee in countries where the use of it is not controlled by the laws; and if a government be seriously disposed to encourage the use of coffee, the Count considers it as indispensable that individuals should be permitted to roast it in their own houses. But as the roasting and grinding of coffee takes up considerable time, the author describes a contrivance of a canister to keep it in, which has a double cover. This vessel is a cylinder of tin, having a sliding piston within, of the same material, formed like the cover of a box, but having several slits in its sides, by which they are sprung outwards and cause it to retain its place in the cylinder with considerable force. The piston, being pressed down upon the coffee retains it and defends it from the air, while the same object is more completely secured by a common well fitted cover at top. It may be here remarked—that this kind of canister has the advantage of confining the article without including any air in the same space, except what may be diffused between the particles; ---but that, with this exception, a well-corked bottle or other fit vessel may answer the same purpose.

Preparation of the beverage from coffee.

After giving instructions for roasting the coffee and keeping it for use when ground, the preparation of the liquor constitutes the next subject of inquiry. Why this should be so uncertain can only be explained by reference to the circumstances on which those qualities depend which are most esteemed in coffee.

A peculiar aromatic substance extracted by boiling water.

Boiling hot water extracts from coffee which has been properly roasted and ground, an aromatic substance of an exquisite flavour, together with a considerable quantity of astringent matter of a bitter, but very agreeable taste; but this aromatic substance, which is supposed to be an oil, is extremely volatile; and is so feebly united to the water that it escapes into the air with great facility.

which is volatile and soon flies off.

If a cup of the very best coffee prepared in the highest perfection, and boiling hot, be placed on a table in the middle of a room, and suffered to cool, it will, in cooling, fill the room with its fragrance; but the coffee, after having become cold, will be found to have lost a great deal of its flavour. If it be again heated, its taste and flavour will be still farther impaired; and after it has been heated and cooled two or three times, it will be found to be quite vapid and disgusting.

Upon this the exhilarating quality depends.

The fragrance diffused through the air is a proof, that the coffee has lost some of its most volatile parts; and as that liquor is found to have lost its peculiar flavour, and also its exhilarating quality, it is inferred, that both these qualities must undoubtedly depend on the preservation of those volatile parts which so readily escape.

It would not escape if the hot liquid had no agitation.

If the liquid were perfectly at rest, the particles which could escape from its surface, would be incomparably less in quantity, than would escape by agitation, which would continually present new portions of the fluid to the air. But all fluids, while heating or cooling, by partial communication, are known to be agitated; a fact long and well known, but particularly explained and insisted upon by our author, in many of his valuable works, and which he again perspicuously and familiarly explains in the present essay. His object is to indicate by what means the heat of the liquor may be uniformly kept up in all its parts: for the consequence being, that the parts will, in those circumstances, be at rest, the motions by which the aromatic parts might have been dissipated, will not take place.

Agitation may be prevented by surrounding the vessel with boiling water or steam.

By pouring boiling water on the coffee, and surrounding the containing vessel with boiling water, or with the steam of boiling water, the coffee itself will be kept permanently at the same heat, and will not circulate, or be agitated.

Coffee requires boiling hot water.

The Count observes, that from the well-known fact, that boiling water is not the most favourable for* extracting the saccharine parts from malt in brewing, he was induced to try a lower temperature than the boiling heat in making coffee; but the coffee did not prove so good. The cold infusion of coffee, which he also tried, was of very inferior quality.

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* I have always understood, that the temperature of boiling is no otherwise exceptionable in brewing, than because it *makes a pudding*; which phrase denotes, that the grains are rendered so adherent to each other, by the sudden and complete extrication of mucilage, that the wort cannot run off.—N.

The common method is wasteful and bad.

The common method of boiling coffee in a coffee pot, is neither economical nor judicious. A large quantity of the material is wasted in this method, and more than half of the aromatic parts, so essential to its good qualities, are lost.

One pound of coffee will make 56 cups.

One pound of good Mocha coffee, which, when properly roasted and ground, weighs only fourteen ounces, will make, by proper management, fifty-six full cups of the very best coffee that can be made.

It must be finely ground.

If it be not ground finely, the surfaces of the particles only will be acted upon by the hot water, and the waste will be very great, from the large proportion of coffee left in the grounds.

A coffee cup contains one gill.

The size of a coffee cup in England usually answers to 8⅓ cubic inches, but the Count considers the *gill measure* as a proper standard for *a cup of coffee*, which he therefore adopts. This will fill the former cup to seven-eighths of its capacity, and *a quarter of an ounce* of ground coffee will be fully sufficient to make a gill of the most excellent coffee.

Coffee must be made by percolation, and not by decoction.

The vessels and their dimensions.

It is well known to chemists, that any solvent already in part charged with a substance intended to be taken up, will be less disposed than before to take up any additional quantity; and upon this is founded the process of percolation or straining, as is practised in brewing and other arts, and has been for some time recommended and used in making coffee. To this the Count gives his approbation. He finds, by experience, that the stratum of ground coffee to be laid upon a perforated metallic bottom of a vessel or strainer, ought to be about two-thirds of an inch thick, and to be reduced by pressure by a piston or flat plate of metal (after levelling it) to less than half an inch. From the data he infers, by a chain of observations, that if the height of a cylindrical vessel or strainer be taken constantly at 5½ inches, the diameter of its bottom must be—To make 1 cup of coffee = 1½ inch—2 cups = 2⅛—3 or 4 cups = 2¾—5 or 6 = 3½—7 or 8 = 4—9 or 10 = 4⅝—11 or 12 = 5.

A strainer, a reservoir, and a surrounding boiler.

These strainers are to be suspended in their reservoirs or vessels for containing the coffee, and the whole included in another vessel called the boiler, which is to contain boiling water, kept hot by a lamp, or otherwise. The forms of these are given with drawings, upon which it does not seem needful to enlarge in the present abridgment, because there are several vessels of this description, with the exception of the surrounding boiler, to be found in our shops.

Description of a very simple and cheap apparatus for making coffee.

The reader must have recourse to the essay itself for these and other particulars of considerable interest, and delivered in the familiar and perspicuous style which distinguish the writings of this author. The poor, and those who prefer simplicity of structure to the extremes of perfection, will be gratified by a description of his last apparatus, fig. 8. It is a porcelain, or earthern jug, with a tubular spout, not unlike those which we call milk jugs, except that these commonly have a lip-spout (which would answer nearly as well.) Into the mouth of this is fitted a tin vessel, which fits and descends a little way down. It has a flat bottom perforated with many holes, and a good close cover; and it would be well to have a round plate or rammer, to compress the coffee on its bottom, and defend it from the stream of hot water, when poured in. These several parts are to be dipped in boiling water before using, and the difference between coffee made by this simple and cheap apparatus, of which the mug may also be applied to other uses, and that made by the the most perfect machines, will scarcely be distinguishable.

General considerations or hints respecting the benefits of coffee.

Sufficient length has already been given to our abstract, to forbid us to follow the Count in the explanation of his views directed to the b. nefit of society, with relation to the comforts of individuals, as well as to the economy of the political aggregate. That it would be preferable to consume an article produced by the colonies of European nations, who demand the manufactures and products of the parent state, instead of sending bullion to China for an article of less value: that it would be preferable that the poor should enjoy the innocent exhilaration of coffee, and the nutriment of sugar, instead of forgetting their hardships during the momentary intervals of insanity, produced by fermented and distilled liquors; that they should be cheerful, benevolent, animated, healthy, and industrious with coffee, instead of becoming outrageous, mischievous, diseased, idle, and sunk in languor and debility with gin, &c. &c. These are among the meditations interspersed through this little work, which the reader will be gratified in consulting, and will probably be induced to make others in his turn.

XIV.

METEOROLOGICAL JOURNAL.

1812.	Wind	BAROMETER. Max.	Min.	Med.	THERMOMETER. Max.	Min.	Med.	Evap.	Rain	
10th Mo.										
Oct. 27	S W	29·40	29·24	29·320	52	40	46·0	—	·26	☾
28	S W	29·75	29·40	29·575	51	32	41·5	—		
29	W	29·80	29·74	29·770	49	33	41·0	·15		
30	S E	29·78	29·66	29·720	51	39	45·0	—	·14	
31	W	29·94	29·78	29·860	54	41	47·5	—		
11th Mo										
Nov. 1	S W	29·94	29·87	29·905	55	50	52·5	—	—	
2	N	30·05	29·87	29·960	54	44	49·0	·7	·65	
3	S W	——	——	——	49	38	43·5	—		
4	W	30·05	29·83	29·940	48	39	43·5	8		●
5	N W	29·83	29·80	29·815	49	30	39·5	—		
6	S W	29·83	29·80	29·815	45	27	36·0	—		
7	W	29·83	29·79	29·810	45	24	34·5	—		
8	E	29·79	29·77	29·780	40	27	33·5	5		
9	E	——	——	——	46	39	42·5	—		
10	N E	30·15	30·03	30·090	45	33	39·0	—		
11	S E	——	——	——	46	39	42·5	—	·32	
12	E	30·03	29·58	29·805	45	39	42·0	—	·38	◗
13	N E	29·58	29·20	29·390	51	44	47·5	—	·55	
14	W	29·59	29·20	29·395	52	40	46·0	—	—	
15	S W	29·59	29·30	29·445	52	39	45·5	—	3	
16	N E	29·30	29·00	29·150	46	42	44·0	—	—	
17	N E	29·00	28·96	28·980	46	40	43·0	—	—	
18	N W	29·66	28·96	29·310	42	32	37·0	7	·13	☽
19	N	29·86	29·66	29·760	42	28	35·0	—		
20	N	29·97	29·83	29·900	41	33	37·0	—	—	
21	N E	30·32	29·97	30·145	39	26	32·5	7		
22	N	30·38	30·31	30·345	43	25	34·0	—		
23	S W	30·31	30·08	30·195	44	26	35·0	—		
24	S W	30·08	29·89	29·985	48	39	43·5	9		
		30·38	28·96	29·678	55	24	41·31	0·58	2·46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Tenth Month, 27. Misty and overcast a. m.; wet at noon: p. m. the Barometer descended at the rate of a tenth of an inch per hour, the wind increasing in proportion, with much rain, the clouds sweeping the earth. The evening was very tempestuous; before midnight the Barometer had risen again and the weather was moderate. Many large trees were blown down. 28. a. m. hoar frost rather misty. At sunset, the sky exhibited a fine collection of coloured clouds, in the modifications *Nimbus* and *Cirrus*, with broad parallel bands of red in the haze above them. 28. Fair and calm. 30. *Cirrostratus* and *Cumulus*: the sky again beautifully coloured.

Eleventh Month. 1. Cloudy. 2. a. m. wet. 5. Fine day. 6. 8, 9, 10, 11. Chiefly misty or cloudy, with hoar frost, and some very thick local fogs. 11. Overcast, a. m. The *Cirrostratus* prevails, and sounds travel with the wind to an unusual distance: we hear the rattling of the carriages on the pavement in London through a direct mean distance of five miles. This phenomenon is to be attributed to a thick continuous sheet of haze in the air above us which acts as a sounding board. 12. Rain through the day. 13. Misty: rain: sounds are again distinctly heard from the city. 15. Fair: a *Stratus* at night. 16. Overcast; with an easterly gale. 18. Wet stormy day, night clear and calm. 20. Misty, much rime on the trees, which came off about noon in showers of ice. At 11 a. m. a perfect but colourless *bow* in the *mist:* near 4 p. m. there was a shower, in which the rainbow shewed its proper colours. 22. Clear: the ground just sprinkled with hail balls. 23. a. m. misty rime, p. m. clearer, thaw in the night. 24. Clear morning.

RESULTS.

Winds for the greater part westerly; though the rain chiefly fell during an easterly wind.

Barometer: highest observation, 30·38 inches; lowest 28·96 inches; Mean of the period 29·678 inches.

Thermometer: highest 55°; lowest 21°. mean 41·31°.

Evaporation 0·58 inches. Rain 2·46 inches.

PLAISTOW, **L. HOWARD.**

***Twelfth Month*, 18, 1812.**

XV.

Description of an improved Pump for raising the Water from Wells or Mines, while sinking or making. By Mr. WILLIAM BRUNTON, *of Butterley Iron Works, in Derbyshire**. *Extracted from the Transactions of the Society of Arts, published in the Year* 1812.

Improved pump for sinking of wells, mine-shafts, &c.

THE contriver of this pump, previous to entering upon a description of his drawings, gives the following statement of the inconveniences he proposes to obviate.

First, as it is necessary for the pumps, whilst sinking, to be always working upon air, that the water may be kept very low in the pit, the engine of course frequently goes too fast, and carries up, by the violence of the current, small pieces of stone, coal, or other substances, and lodges them above the bucket, which must considerably retard the working of the pump, and wear the leather.

Secondly, When the engine is set to work, (after having been stopt whilst working upon air, and consequently a quantity of air remaining in the suction-pipe, with the small stones, &c. deposited on the valves of the bucket) it often happens, that the compression of the air, by the descent of the bucket, is not sufficient to overcome the weight of the bucket valves so loaded with rubbish, and the column of water in the stand pipes, the pump is hereby prevented from catching its water; the usual remedy for which is, to draw the bucket out of the working barrel, until a quantity of water has escaped by its sides, and displaced the air. Observe here, that this often happens from the unnecessary magnitude of the space between the bucket and clack.

Thirdly, The pumps are suspended in the pit by capstan ropes, for the purpose of being readily lowered as the pit is sunk; the stretching of the ropes, (especially when sinking in soft strata,) occasions much trouble, by suffering the pumps to choke; but the most serious evil is, that the sinkers, in shifting the pumps from one place to another, throw them very far out of perpendicular, thereby causing immense friction, and wearing in all the parts; besides endangering the whole apparatus, by breaking the bolts and stays, and straining the joints.

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* For which the silver medal was voted.

Improved pump for sinking of wells, mine-shafts, &c.

Fourthly, As the pumps sink, the delivering pipe at the top is raised, by putting on short pipes, generally about a yard at a time, which occasions many stoppages and much hindrance in the work.

Having an engine pit to sink at Codnor Park Colliery, Derbyshire, belonging to the Butterley Iron Company, I endeavoured to obviate the difficulties stated; and first, for the purpose of preventing the pumps working too much upon air, I constructed a working barrel, (which in this case was nine inches diameter,) with a side pipe three inches diameter, connected therewith by an opening at the top and bottom; also at the upper end of the side pipe I fixed a valve, so as to slide over and shut the communication with the working barrel, the stem of the valve by which it is regulated, passing through a stuffing box, and by letting a quantity of water return through the side pipe to the bottom of the working barrel, (the men at the bottom regulating the valve, so that the pump takes the water as it comes,) very little rubbish is then taken into the pump, and much wear and tear of buckets prevented.

Secondly, I also, by this valve and side pipe, preclude the necessity of ever drawing the bucket to displace the air. The clack piece was made as small as possible, and the clack with its gearing very low, in order to have as little space as possible between the bucket and the clack. The clack, as represented in the drawing, possesses the advantages of being easily caught by the clack hook in case of being under water. The ring prevents it from oversetting, and thereby fastening itself in the pumps, and the valves are very easily repaired by unscrewing the cross-bar, which admits of their being taken off and replaced.

Thirdly, I avoid the inconvenience of suspending the pumps by ropes, by forming the suction-pipe in two pieces, one inner and outer pipe; the outer pipe is bored for about six inches in length, and the inner one turned cylindrically to fit it; they slide into each other the whole length of a regular pipe, viz. nine feet; and they are made tight by collars of leather, surrounded by a cup filled with water and clay. The

Improved pump for sinking of wells, mine-shafts, &c.

pumps are supported at proper distances, so as to suit the length of the pipes, by beams, and across those are other beams, upon which the flanches of the pipes rest: these last are not fastened by any bolt, in order that they may be readily removed. The pumps, by these means, remain stationary, and the suction-pipe lengthens as the pit is sunk, until it is drawn out to its full extent. The whole column is then lowered to the next flanches, and another pipe is added to the top; the lower end of the suction-pipe is formed somewhat like a crank, in order that the sinkers, by turning it round upon the other pipe, may move it from one place to another, and so prevent the necessity of sinking immediately under it.

Fourthly, The pumps being stationary, as above stated, the pipe at the top will of course deliver the water at the same level at all times, and instead of being obliged to lengthen the column every yard sunk, it will only be necessary every nine feet.

Fig. 1. Pl. II, is the section of a shaft or pit, with the pump fixed in it; it is cast in lengths of nine feet each, screwed together by flanches, and supported by beams extending across the pit, (as shown in the plan, fig. 6,): short pieces are laid across these, with half circular holes in them; and these being put round the pump, just beneath a flanch, sustain the pump firmly, but may quickly be removed when it is required to lower the pumps in the pit; and, as they are not fastened, they do not prevent the pumps being drawn upwards; A, fig. 1, is a door which unscrews, to get at the lower valve or clack of the pump; this is more clearly explained in the enlarged section, fig. 2, where A has the same designation, B, fig. 2, is the working barrel, with the bucket D working in it; E is the clack, also shewn enlarged in figs. 3 and 4; F is the suction-pipe, and GG the moveable lengthening piece; this slides over, and includes the other, as in fig. 2, when the pump is first fixed; but, as the pit is sunk, it slides down over the pipe F, to reach the bottom, as in fig. 1; the outside of the inner pipe F is turned true and smooth, and the inside of the outer pipe G, at the upper end, is bored out to fit it; the junction is made perfect by leathers placed in the bottom of the cup *a a*, which holds water and wet clay over them, to keep them wet and pliable, and consequently air-tight; the lower extremity of the suction-pipe G, terminates in a nose, pierced with a number of small holes, that it may not take up the dirt: this nose is not placed in a line with the pipe, but curved to one side of it, so as to describe a circle when turned round; by this means the sinkers can always place the nose in the deepest part of the pit, as shewn in fig. 1; and when they dig or blast a deeper part, they turn the nose about into it, the sliding tube lengthening down to reach the bottom of it: by this means there is never a necessity to set a shot for blasting so near the pump foot, as to put it in any danger of being injured by the explosion, as is the case in the common pump; in which this danger can only be avoided by moving the pump foot to one side of the pit, which necessarily throws the whole column of pumps out of the perpendicular.

Improved pump for sinking of wells, mine-shafts, &c.

The construction of the clack is explained by figs. 3 and 4, the former being a section, and the latter a plan; LL is a cast-iron ring, fitting into a conical seat in the bottom of the chamber of the pump, as shewn in fig. 2; it has two stems, *l l*, rising from it, to support a second iron ring, M M; just beneath this, a bar, *m*, extends across from one stem to another, and has two screws tapped through it; these press down a second cross-bar, *n*, which presses the leather of the valves down upon the cross-bar of the ring L, and this holds it fast, forming the hinge on which the double valves open, without the necessity of making any holes through the leather, as in common; but the chief advantage is, that, by this means the clack can be repaired, and a new leather put in, in far less time than at present, an object of the greatest importance; for, in many situations, the water gathers so fast in the pit, that if the clack fails, and cannot be quickly repaired, the water rises above the clack-door, so as to prevent any access to it, and there is no remedy, in the common pump, but drawing up the whole pile of pumps, which is a most tedious and expensive operation. In Mr. Brunton's pump, the clack can at any time be drawn out of the pump, by first drawing out the bucket, and letting down an iron prong, fig. 5, which has hooks on the outsides of its two points; this, when dropped down, will fall into the ring M, and its prongs springing out, will catch the underside, and hold it fast enough to draw it up; another part of Mr. Brunton's improvement consists in the addi-

Improved pump for sinking of wells, mine-shafts, &c.

addition of a pipe H, fig. 2, which is cast at the same time with the barrel, and communicates with it both at the top and at the bottom, just above the clack; at the upper end the pipe is covered by a flat sliding plate, which can be moved by a small rod, *l*, passing through a collar of leather; the rod has a communication by a lever, so that the valve can be opened or shut by the men in the bottom of the pit; the object of this side pipe is, to let down such a proportion of the water, which the pump draws, as will prevent the pump drawing air; though of course the motion of the engine will be so adapted, as not to require a great proportion of the water to be thus returned through the side pipe; yet it will not be possible to work the engine so correctly, as not to draw some air, without this contrivance; and if it does, it draws up much dirt and pieces of stone into the pump, besides causing the engine to work very irregularly, in consequence of partially losing its load every time the air enters the pump. Another use of the side pipe is, to let down water into the chamber of the clack to fill it, when the engine is first set to work, after the pumps have been standing still, and the lower part of the barrel and chamber empty*.

XV.

An Account of an Experiment made in the College Laboratory, Edinburgh, drawn up by JOHN DAVY, *Esq.*

SIR,

If water be not shewn by the combination of mur. a gas, and amm. gas, *the mur. a gas contains none.*

IN the preceding numbers of your Journal, several papers have appeared, relative to the result of the combination of muriatic acid and ammonia. Mr. Murray first made the experiment, for the purpose of ascertaining the nature of the former gas—whether it be a compound of an unknown basis and water, or a compound of chlorine and hydrogene. He justly concluded, that if water was obtained from muriatic acid gas, by means of ammonia, its existence in the acid must be admitted; and that, on the contrary, if no water could be procured, it would be unphilosophical to suppose water present; but that muriatic acid gas must be considered as a compound of hydrogene and chlorine. Such were Mr. Murray's premises.

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* This communication was accompanied by a handsome letter from that eminent civil engineer, William Jessop, Esq., who, after explaining the usual practice and the effect of Mr. Brunton's improvement, adds, that simple as it is, it will be found, as he has from experience ascertained, to be of considerable value to those interested in mining concerns.

The result of his experiment, he says, was the production of water from the muriate of ammonia, formed by the union of the dry gases. He therefore, of course, concluded, that muriatic acid gas is not a compound of chlorine and hydrogene, but of water and an unknown basis (muriatic acid;) in fact, that the old doctrines respecting this substance, which he had strenuously defended before, are correct, and the new theory advanced by Sir H. Davy, erroneous.

Asserted fact by Mr. Murray, that the dry gases afford water. Conclusion, *that m. a. gas is water and a basis.*

This experiment was also repeated on a very small scale at Liverpool, by Drs. Bostock and Traill, and with nearly the same result.

But other results have been obtained.—Sir H. Davy has made the experiment several times, and under different circumstances; and has uniformly perceived no water, when the atmospheric air was excluded, and dry vessels, and dry gases were employed; and my experience is agreeable to his, having been unable to obtain any water the only time I repeated the experiment, on subjecting the muriate to a heat not sufficient for its sublimation, though water was procured by heating the same salt, after it had been exposed to the atmosphere.

Sir H. Davy and the writer obtained no water.

It is not my object at present to attempt to reconcile these contradictory results, or to show, by any process of reasoning or criticism, that Mr. Murray has fallen into errour. It is my intention to confine myself to the concise detail of new experiments, which will tend, I trust, to decide the question.

About two months since, when my brother, Sir H. Davy, was in Edinburgh, he was desirous of repeating the experiment on the combination of muriatic acid and ammonia, with Dr. Hope. The experiment was accordingly made in the College Laboratory.—Sir George Mackenzie, Mr. Playfair, and some other gentlemen, were present.

Repetition of the experiment before eminent men.

The alkaline and acid gases employed, were pure, and both had previously been dried by exposure, for about sixteen hours, to

The amm. gas was dried by

potash; the mur. a. gas by mur. of lime. They were in alternate portions received in an exhausted vessel. The mur. amm. was driven from the neck of the vessel; and this being cooled, and the body heated,

substances having a strong attraction for water—the ammoniacal gas to pieces of potash—and the muriatic to dry muriate of lime. The apparatus for making the experiment consisted of a plain retort of about the capacity of twenty-six cubic inch measures, with a stop-cock, and of a receiver, with a suitable stop-cock. The latter was filled over mercury with one of the gases, which from the receiver passed into the exhausted retort, by means of the stop-cocks; the other gas was introduced the same way into the retort; and thus alternately about ninety cubic inches of each gas were combined. The muriate of ammonia formed, was of its usual appearance. As it was diffused over the whole surface of the retort, it was necessary to clear the neck by the sublimation of the salt into the bulb, that if any water was present, it might be detected here in the second part of the operation.

All the salt being driven into the bulb of the retort, by the heat of a spirit lamp, the neck was cooled and kept cold by moistened cloths, whilst the bulb was heated by a coke-fire, till the muriate began to sublime, and to make its appearance at the curvature of the vessel. The fire was now withdrawn. It had been gradually and equally applied, and it had been continued for a considerable time.

a dew, just perceptible, was seen in the neck,

The result was examined whilst the bottom of the retort was still very hot, and whilst that part, where a little of the muriate had sublimed, exceeded the temperature of boiling water. A dew just perceptible was observed lining the cold neck. The quantity of water was so extremely small, that the globular particles composing this dew could scarcely be perceived by the naked eye, unassisted by a magnifying glass.

This result appeared to me very decisive. The quantity of gases employed was large; the water, which ninety cubic inches of muriatic acid gas should afford, is, according to hypothesis, equal to no less than eight grains. How great is the difference between this quantity and a dew barely perceptible! which may reasonably be referred to a minute quantity of vapour in the gases, or to a little moisture derived from the mercury, a small quantity of which entered the retort with the gases.

which seemed about one

Dr. Hope wished to ascertain how much water would produce such a dew as was observed. For this purpose he heated in a retort, of a similar size to that used in the experiment, a single drop of water, which it may be said weighs about 1 grain. The appearance of condensed water in this instance in the neck of the retort, was much greater than in the preceding; he thought that it was 3 or 4 times as great.

sixth of a grain.

Deduction: that water is not a constituent part of mur. a. gas; but that it is a comp. of chlorine and hydrogene.

May we not conclude from these results, on Mr. Murray's own ground of reasoning, that water is not a constituent part of muriatic acid gas, and that this substance is a compound merely of chlorine and hydrogene? And may we not reasonably consider that very minute portion of water, which did appear, as uncombined moisture derived from various sources? It is easy to account for the presence of about $\frac{1}{3}$ of a grain of water on the one theory; it is impossible to account for the absence of 8 grs. on the other.

It has been shewn, by Dr. Henry, that ammonia obstinately retains aqueous vapour; and Sir H. Davy has proved, that a minute portion of solution of muriatic acid in water, may be obtained by intensely cooling the gas. There is great difficulty in drying mercury without boiling it; and in the present instance the mercury was not boiled. These trivial circumstances do not deserve notice, otherwise than as tending to account for the very minute quantity of water obtained. It is probable, judging from the past, that objections will be made, and I wish to anticipate them.

If water had been present it would have risen.

The present mode of heating the muriate of ammonia in a close retort, which had also been adopted on a former occasion, was objected to in a preceding number of your Journal. Mr. Murray there observed, that in consequence of the air being confined, it was possible that the water could not rise in vapour, or at least that it was impeded in its volatilization. His reasoning was subtile, and it would have been plausible had there been no circulation of air in the vessel, and quite correct if the heat employed had not been sufficient to convert the water into an elastic fluid or true gas.—But in a large retort such as we used, there is a circulation of air, when heat is partially applied; and the heat employed was far above that required for boiling water.

Experiment in proof.

Not to dwell on reasonings, which on controverted points are always very justly to be suspected, I shall have recourse to fact. A single drop of water was introduced into a retort, about the same size as that employed in the experiment, and it was tightly stopped by a cork. On the application of heat to the bulb, the water passed off into steam apparently with the same velocity that it would have done, had there been a free communication between it and the atmosphere, and of course the steam was just as readily condensed. This experiment was suggested and made by Dr. Hope.

I have now finished the account of the experiments which I wished to communicate, and as I have no intention of answering personal aspersions, which are only injurious to the author when unjustly made, nor of entering again into a controversy concerning words, I shall here conclude with subscribing myself,

Your obedient humble servant,
JOHN DAVY.

Edinburgh, December 9.

To Mr. Nicholson.

P. S. I have authority from Dr. Hope, and also from Sir George Mackenzie and Mr. Playfair, to mention, that the detail I have given of the experiment is correct.

I should have before observed, that the muriate was heated in the preceding experiment in a partial vacuum. After the combination of the two gases had been formed, a little ammonia remained in the retort, and to this as much air was admitted, as was conceived sufficient when the heat was applied, to produce the common atmospheric pressure.

SCIENTIFIC NEWS.

Account of Books, &c.

Philosophical Transactions of the Royal Society of London, for the year 181[illegible] *Part II. 4to.* 187 *pages, with* 12 *plates.*

THIS part contains the following paper. 1. Observations of a second Comet, with remarks on its construction. By William Herschel, LL. D. F. R. S. 2. Additional Experiments on the Muriatic and Oxymuriatic Acids. By William Henry, M. D. F. R. S. &c. (See our present volume, p. 42.) 3. Of the Attraction of such Solids as are terminated by planes;

and of solids of greatest attraction. By Thomas Knight, Esq. 4. Of the Penetration of an Hemisphere by an indefinite number of equal and similar Cylinders. By Thomas Knight, Esq. 5. On the Motions of the Tendrils of Plants. By Thomas Andrew Knight, Esq. F. R. S. (See our present Vol. p. 37.) 6. Observations on the Measurement of three degrees of the Meridian conducted in England by Lieut. Col. William Mudge. By Don Joseph Rodriguez. 7. An account of some Experiments on different Combinations of Fluoric Acid. By John Davy, Esq. 8. On a Periscopic Camera Obscura and Microscope. By William Hyde Wollaston, M. D. Sec. R. S. (See our present vol. p. 26.) 9. Farther Experiments and Observations on the Influence of the Brain on the generation of animal heat. By B. C. Brodie, F. R. S. 10. On the different Structures and situations of the solvent Glands, in the digestive organs of Birds, according to the nature of their food, and particular modes of life. By Everard Home, Esq. F. R. S. 11. On some Combinations of Phosphorus and Sulphur, and on some other subjects of Medical Inquiry. By Sir H. Davy, Knt. Sec. R. S. List of Presents. Index.

The History of the Royal Society from its Institution to the End of the 18th Century. By Thomas Thomson, M. D. F. R. S. L. and E. 2 vols. 4to. price 2l. 2s. and on fine paper, 3l. 12s.

Mr. Andrew Horn, of Wycombe, acquaints me that he has a short *Essay on Vision* in the press, in which the Seat of Vision is determined, and by the discovery of a new function in the organ of Sight, a foundation is laid for explaining its mechanism and the various phenomena, upon principles hitherto unattempted.

Bionomia. Opinions concerning Life and Health, introductory to a Course of Lectures on the Physiology of Sentient Beings. By A. P. Buchan, M. D. of the Royal College of Physicians, London, 8vo. 119 pages, with 8 p. Introduction.

Where the master of a science, not to be acquired without deep erudition, a diligent and correct observation of facts, and an enlightened spirit of philosophical research, takes his station on an eminence, and by a few striking outlines gives a sketch of the prospects around him, it becomes impossible to make an analysis of his work. I must therefore confine myself to say, that this treatise contains many important and highly interesting truths, delivered with perspicuity and elegance.

M. De Luc's Geological Travels in Germany, France, and Switzerland, are nearly ready for publication.

A work on Oriental Commerce, in two 4to. vols. By Mr. Milburn, with numerous Charts, by Arrowsmith, is expected to be published in a few weeks.

Tyrocinium medicum; or a Dissertation on the Duties of Youth, apprenticed to the Medical profession. By William Chamberlayne, Member of the Royal College of Surgeons, Fellow of the Medical Society of London, &c. duodecimo, 253 pages. London, 1812.

THIS familiar and very perspicuous Dissertation, contains much more than is indicated in the title. It is a subject of primary interest to the public, that the preparation and dispensing of medicines should be done with fidelity, precision, and dispatch. It is of equal importance that the professors of the art should not be deficient in the requisite information. But in every class and every rank of Society, the habits of order, method, cleanliness, punctuality, and other good qualities, which have been called the minor virtues, are so essential to prosperity and happiness, that a book which strikingly displays their advantages, must be considered as of much more extensive utility than any set of Aphorisms confined to an individual profession. The good advice with which this Treatise abounds, is calculated to afford great benefit to the reader, whether intended for the Medical profession, or for any other department of life.

M. Zambeccari, accompanied by a friend, ascended in a balloon from Bologna, on the 21st September. On his descent, the balloon became entangled in the branches of a high tree, and, before it could be disengaged, caught fire. The two aeronauts leaped out. M. Zambeccari was killed upon the spot;

but M. Bologna, his friend, survived, though some of his limbs were broken.

The ascension of Bittorf, the mechanician, from Manheim, was equally disastrous. When he had risen to a considerable height, he perceived, too late, that his balloon was damaged, and he had no other resource than to open the valve. The balloon descended with extreme velocity, and the inflammable matter which it contained, took fire, the shreads of the balloon falling upon M. Bittorf's head and breast, which were much burnt. On a sudden, his crazy vehicle struck upon the roof of a house, two stories high, from which he was precipitated, and died the next day in great agony.

Mr. Sadler, the aeronaut, ascended from Belvidere-house, near Dublin, October 1. at 1 p. m. with the wind at south-west, and in thirty-five minutes had sight of the mountains in Wales; he continued in the same direction till three o'clock, when being nearly over the Isle of Man, the wind blowing fresh, he found himself fast approaching the Welch coast; and at four o'clock, he had a distinct view of the Skerry light-house, and the prospect of consummating his ardent hopes of a speedy arrival in Liverpool. The wind now shifting, he was again taken off, and lost sight of land; when, after hovering about for a long time, he discovered five vessels beating down channel; and in hopes of their assistance, he determined on descending with all possible expedition, and precipitated himself into the sea. In this most critical situation he had the mortification to find the vessels took no notice of him: obliged, therefore, to reascend, he now threw out a quantity of ballast, and quickly regained his situation in the air, to look out for more friendly aid. It was a length of time before he had the satisfaction of discovering any; and then observed a vessel, which gave him to understand by signal, that she intended to assist him, but could not reach him. Two others also now appeared in sight, and one of them tacking about, hoisted the Manx colours: night now coming on, he was determined to avail himself of their friendly aid, and once more descended into the sea; but here the wind acting upon the balloon, as it lay on the water, drew the car with so much velocity, that the vessel could not overtake it; and, notwithstanding

standing he used his utmost efforts, and latterly tied his clothes to the grappling-iron, and sunk them to keep him steady; still the balloon was carried away so fast, that he was under the necessity of expelling the gas; upon that escaping, the car actually sunk, and he had now nothing but the netting to cling to. His perilous situation, and the fear of getting entangled, deterred the men from coming near him; until, being in danger of drowning, Mr. Sadler begged they would run their bowsprit through the balloon, and expel the remaining gas. Having done this, they threw out a line, which he wound round his arm, and was then dragged a considerable way before they could get him on board, quite exhausted.

A meteoric stone, of the weight of 15lbs. fell to the earth on the 1st of March, 1811, in the village of Konleghowsk, dependent on the town of Romea, in the government of Tschernigoff, in Russia, and making part of the domains of Count Golovkin. Its fall was preceded by three violent claps of thunder. When it was dug out from the depth of more than three feet, through a thick layer of ice, it still possessed heat. It was remarked, that at the third clap of thunder there was an extraordinary explosion, with a loud noise, and throwing out a great quantity of sparks.

A new comet was discovered by M. Pons, of Marseilles, on the 20th of July. Its course was then between the feet of the Camel-leopard and the head of the Lynx. It was discovered afterwards at Paris, by M. Bouvard; and, according to the calculations of these astronomers, it passed its perihelion on the 15th of September, when its distance from the sun, taking that of the earth as unity, was at 0,77835, and its inclination to the ecliptic is 74° 50′.

The Geological Society held its first meeting of the present session on Friday, November 6th, 1812.

A second letter from Ed. L. Irton, Esq., in answer to some queries by the President, relative to the sand tubes found at Drigg, in Cumberland, was read.

From this it appears, that the tubes have hitherto been found only in a single hill of drift sand on the sea-shore, of the extent of about five acres. The entire form of the tubes is not known; for they are discovered in consequence of being laid bare by the drifting of the sand; and the same cause almost always breaks off, and injures their upper extremity. The manner in which they terminate below, is still less known: one of the tubes was exposed by hazardous digging, in running sand, to the depth of about fifteen feet, without the least appearance of its being about to terminate. They lie parallel to each other, and nearly vertical, but at unequal distances—the number must be very considerable, Mr. Irton having himself taken away, at different times, not less than a hundred.

The tubes, when first dug out, are very flexible, but exposure to the air for a few seconds deprives them of this quality. The unctuosity of the internal glazing of these tubes, when recently dug up, stated by Mr. Irton, in his first letter on the authority of another person, appears, on more accurate examination, to be a mistake.

A communication from George Cumberland, Esq., relative to some lime-stone strata in the neighbourhood of Bristol, was read.

The strata here described compose the rocks opposite to the Hotwell Walks, and are farther illustrated by two drawings; the one of the external face of the rocks, the other of a large cavern recently discovered. In clearing the ground for the erection of houses opposite to the Old York hotel, on Clifton downs, some interesting varieties of sulphate of strontian were met with, but the place being now covered with building and garden grounds, there is little likelihood of its being soon again opened to the researches of the mineralogist.

A communication, accompanied by three drawings in illustration, from Dr. Mac Culloch, Mem. G. S. relative to a remarkable interrupted vein in lime-stone, was read.

This vein occurs in a mill-stone which was shipped from Limerick, and is at present at the royal powder mills at Waltham Abbey. The stone itself is a dark blue slaty limestone, containing comminuted fragments of marine remains; the vein by which it is traversed is whitish compact carbonate of lime. This vein, in its present state, consists of a number of separate angular fragments, having somewhat of a general parallelism with such a correspondence at any two neighbouring extremities as to render it a matter past doubt that they have once formed a continuous vein.

To displace such a vein into its present position, it is necessary to suppose that the rock originally consisted of a series of very thin strata, which, being fissured across, formed a space for the reception of the substance of the vein. It is evident from the angularity and the irregularly-serrated edges of the displaced fragments, that the white calcareous carbonate must have been perfectly indurated at the time of its displacement: yet that the strata of the limestone must have been in a state to admit of a series of shifts or slides, each successively advancing with equal intervals beyond the one preceding it: it is necessary also to suppose that the strata must have been in some condition admitting them to cohere intimately together, either at the period when the slides took place, or afterwards, from the perfect obliteration of the seam. By what theory can these facts be explained?

Friday, Nov. 20.

A communication from Ar. Aikin, Esq. Sec. entitled " Some observations on a bed of Greenstone, near Walsall, Staffordshire, was read.

The Greenstone, which is the subject of this paper, is of a dark blackish-blue green colour, has a glimmering lustre, and an uneven fracture, breaking into irregularly wedge-shaped blunt-edged fragments: it is tough, acquiring a kind of polish under the hammer, is moderately hard, and rather heavy. It strongly attracts the magnetic needle, and effervesces on immersion in cold diluted muriatic acid. It consists principally of felspar, mixed with cal[illegible]eous spar, with minute shining black grains of Augite, and of hornblende. It is penetrated by nearly vertical contemporaneous slender veins of calcareous spar, and after a few weeks exposure to the air acquires a liver-brown colour and falls to pieces.

It occurs in the independent coal formation; but is not coextensive with this formation; nor indeed in the opinion of the author of the paper is it to be considered as a true bed, but rather a lateral vein branching off from a large dyke of greenstone that comes up to the surface, dividing the colliery in which the greenstone bed is, from another adjacent to it.

On comparing the strata above and below the greenstone, with the very same strata that have been pierced through in a part of the colliery where the greenstone does not occur, it appears, that the bed of slaty clay with balls of ironstone lying *upon* the greenstone, does not materially differ from the same bed where the greenstone is absent, but that the beds immediately *below* the greenstone, present very different characters where they are covered by this latter from what they do where the contrary is the case. These beds are 1. Sandstone, 2 Bituminous shale, with slender seams of coal, and 3. a coal somewhat more than a yard thick. Of these the sandstone is considerably indurated, the bituminous shale is also indurated, entirely deprived of bitumen, and is broken more or less into irregular pieces, and mixed with the lower part of the sandstone bed. The yard coal is also entirely deprived of bitumen, is stained and iridescent on the surface of its natural joints, and is more friable. These changes appear to accompany the superposition of the greenstone bed through its whole extent, and from the circumstance of their ceasing where the greenstone terminates, they appear to be occasioned in some way or other by this bed.

Scientific Institution, Princes Street, Cavendish Square.

On Tuesday, Jan. 5, Mr. Singer will begin his course of twelve Lectures on Electricity and Electro-chemical science, which will be continued upon each subsequent Friday and Tuesday, until concluded. And on the 23d of Feb. he will begin his course of Voltaic Electricity. In addition to the extensive apparatus before employed, he has now in forwardness an entire new Battery of one thousand double plates, with a variety of auxiliary apparatus.

Anatomical Theatre; Lower College Street, Bristol.

Mr. Thomas Shute will commence his spring course of Lectures on Anatomy, Physiology, and the principles and operations of Surgery, on Saturday the 8th January, at eight in the morning.

Dr. Buxton will commence his spring Course of Lectures on the Practice of Medicine, about the 20th of January next.

The Pontine Marshes.

It is announced, from the Continent, that the French have succeeded in draining the Pontine Marshes; a pestilential nuisance which has subsisted for so many centuries, in the vicinity of Rome, in defiance of every attempt of the ancient Imperial, as well as of the papal government. This district, once so healthy and so populous, and at length again reclaimed, is said to afford a disposeable quantity of 150,000 acres of excellent land. The means adopted are not, nor perhaps can be, clearly stated in a short notice. That the Engineers have improved the line; regulated the falls; enlarged the water ways; secured the embankments, sluices, and other works; and no doubt, employed the powers of steam to facilitate their general and particular labours—may be concluded from the science and activity of a people, too long employed in the works of destruction. To works like the present every friend to humanity must join in wishing success and duration.

WILLIAM Davis's Treatise on Land Surveying, to which are now first added a supplement, and a portrait of the Author, the fifth edition greatly improved, enlarged and better arranged is nearly ready for publication.

MR. BAKEWELL will commence a course of Lectures on *Geology* and *Mineralogy*, at the Surry Institution, in January, 1813.

Mr. Nicholson takes this opportunity to acquaint his Patrons and Correspondents, that he has been, for some time, occupied upon such arrangements, with regard to his public undertakings and other concerns, as have enabled him to take the conducting and editing of this Journal entirely into his own hands; which, for some time past, have, in a great measure, been committed to an eminent and able scientific gentleman, who is not at present engaged in the work. The whole of the annotations and remarks, together with various original as well as abridged and selected articles, on different subjects, will consequently, as in times past, be produced by Mr. Nicholson; and he looks forward with confidence and pleasure to many a renewed correspondence on the subjects of natural Philosophy and the Arts.

Mr. Brunton's Improved Pump for Mines.

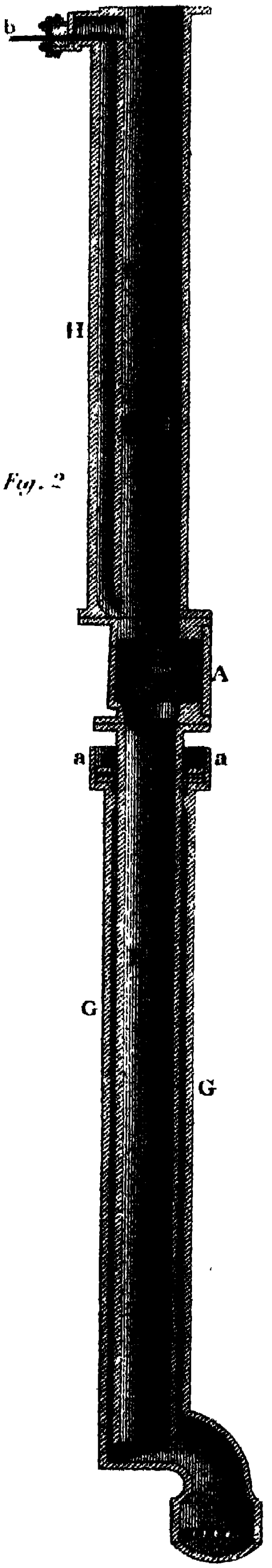


Fig. 2

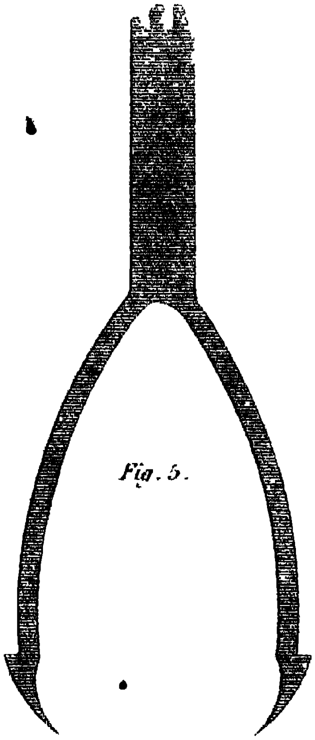
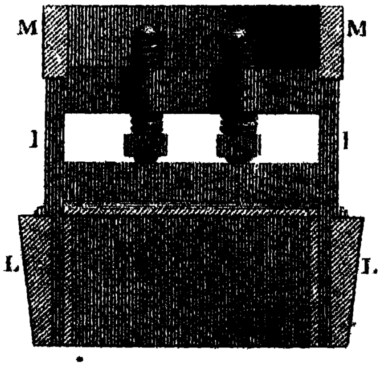
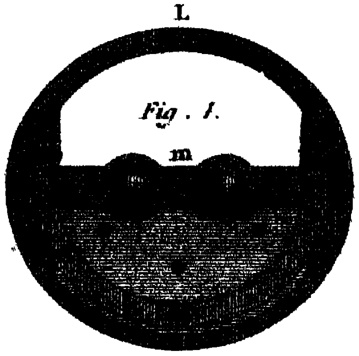


Fig. 5.

Fig. 3.

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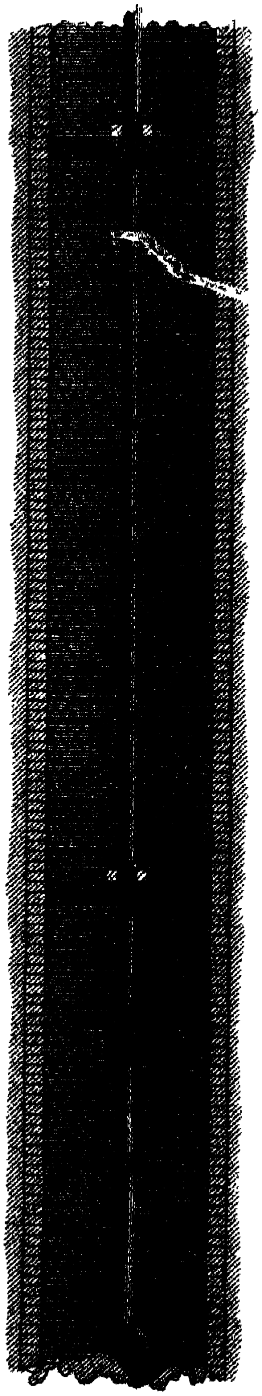
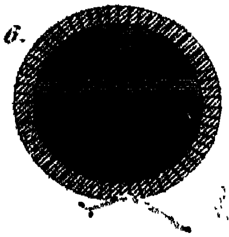


Fig 6.



H. Cooper sculp

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1813.

ARTICLE I.

An Account of some Experiments on different Combinations of Fluoric Acid. By JOHN DAVY, *Esq. From the Philosophical Transactions,* 1812.

Introduction.

Statement of the subject.

TWO years ago, I engaged, at the request of my brother, Sir H. Davy, in an inquiry respecting the nature of common fluoric acid gas. My principal object was to ascertain whether silex is essential to its constitution, and whether the proportion is constantly the same. This subject, and experiments on the fluoric and fluoboracic acids, occupied me for about six months. Since that time, the work of MM. Gay Lussac and Thenard has appeared, entitled "Recherches Physico-Chimiques," in the second volume of which is an elaborate dissertation on fluoric acid. These philosophers, I find, have anticipated many of my results, and consequently very much abridged my labour of detail in the following pages. To repeat what is already known would be useless; I shall therefore confine myself to describe what I have observed, which appears to me yet novel, or different from the observations of the French chemists. The order which I shall pursue, will be that which I observed in my experiments. I shall divide what I have to advance into four parts. The first part will relate to the silicated fluoric acid gas, and to the subsilicated fluoric acid; the second to the combinations of these acids, and of pure fluoric acid with ammonia; the third to fluoboracic acid; and the fourth to its ammoniacal salts.

SECT. 1. *On silicated fluoric acid Gas, and subsilicated fluoric Acid.*

Fluoric acid gas requires either silex or boracic acid to admit of that *state*.

Common fl. a. g. is saturated.

The facts which have already been published by MM. Gay Lussac and Thenard and others, appear to me to be sufficient to prove that pure fluoric acid has not yet been obtained in the gaseous state, and that silex, or boracic acid, is requisite that it may assume this form. Were more evidences necessary, I could advance many in point. One circumstance only I shall mention, proving that common fluoric acid gas is perfectly saturated with silex. I have preserved this gas, made by heating, in a glass retort, a mixture of fluor spar and sulphuric acid, for several weeks over mercury in a glass receiver uncoated with wax, without observing the slightest erosion to be produced.*

It is best obtained by heating fluor spar, finely powdered glass, and sulph. acid together.

This gas, with great propriety, has lately been called silicated fluoric. Before I proceed to its analysis, I shall notice what method I have found the best for obtaining it. I have, for a considerable time, long before MM. Gay Lussac and Thenard's work was published, added to the mixture of fluor spar and sulphuric acid, a quantity of finely pounded glass, and have thus procured the gas with the greatest facility. The advantages of this addition are considerable. The retort is saved, which otherwise, in less than one operation, would be destroyed; and a much larger quantity of gas is procured from the same materials, and with less trouble and less heat; the action indeed at first is so powerful, that gas begins to come over before the application of heat is made, and a very gentle one only is required to continue its production.

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* The sides of the receiver indeed became obscure; but this was not from erosion, but from deposition, as appeared from the transparency and polish of the glass being readily restored by slight friction. What the deposition was, I am ignorant of. After several weeks it was so trifling, as to give only a slight degree of opacity to the receiver.

Common (or silicated) fl. a. gas is 3½ times heavier than water.

Previous to its analysis, it was necessary to ascertain the specific gravity of the gas. This I have endeavoured to do. The gas, the subject of experiment, was quite pure, being totally condensed by water. A Florence flask was exhausted; in this state, weighed by a very delicate balance, it was

= 1452·2 grains.
Filled with common air - = 1452·2 + 10·2
Again exhausted - - = 1452·2
Filled with silicated fluoric gas = 1452·2 + 36·45

Hence as 10·2 : 31 :: 36·45 : : 110·78

Thus it appears, that 100 cubic inches of silicated fluoric acid gas, at ordinary temperature and pressure, are equal to 110·78 grains.

By precipitating the silex by water and amonia, the gas was found to contain 5 parts acid and 8 silex.

When silicated fluoric acid gas is condensed by water, it is well known that part only of the silex is deposited. To obtain the whole, in order to ascertain the proportion in the gas, I have employed ammonia in excess. 40 cubic inches of the gas (barom. 30, therm. 60) were transferred in portions of 10 cubic inches at a time to a solution of ammonia. The silex precipitated was carefully collected on a filter, and washed till the water that passed through it, ceased to be affected by nitrate of lime. It was next dried, and strongly heated in a platina crucible. It weighed 27·2 grains, and was pure silex. Supposing fluoric acid to be the remaining 17·1 grains, which added to 27·2 grains are equivalent to the weight of 40 cubic inches of the gas, it appears that 100 parts by weight of this gas consist of

61·4 silex
38·6 fluoric acid

100·0

That this estimate may be correct, it is evident, that ammonia should have the property of precipitating the whole of the silex of silicated fluoric gas; which I shall not now endeavour to prove, but leave it to be considered in another part of the paper.

The gas contained no alkali.

There is no improbability attached to the idea, that silicated fluoric acid gas may, from the manner in which it is prepared, contain a proportion of alkali. To discover whether this was the case, a solution of nitrate of lime was added to the ammoniacal solution neutralized by nitric acid, from which the silex in the preceding experiment had been removed. The precipitate of fluat of lime was separated by filtration. The filtered liquid was evaporated to dryness; and the ammoniacal salt heated in a platina crucible till it was entirely dissipated. The residue had the appearance and taste of quick lime. It was dissolved in acetic acid, and the solution yielded sulphat of lime on the addition of sulphat of ammonia. The liquid was evaporated to dryness, and when the residuum has been heated to dull redness, nothing remained but a little white powder, weighing about a grain, and having all the properties of gypsum. Thus it appears that silicated fluoric acid gas contains no alkali.

Common liquid fluoric acid,

or subsilicated fluoric acid.

My next object was to ascertain the composition of common liquid fluoric acid—that acid obtained by the decomposition of silicated fluoric acid gas by water, and which, on account of the separation that occurs of part of the silex, may, with greater propriety, be called subsilicated fluoric acid. For this purpose, 43·21 cubic inches, barom. 30·4, therm. 50, or 44 cubic inches at common temperature and pressure, were successively added, two cubic inches at a time, to one cubic inch of distilled water in a small jar over mercury. The whole of this, the gas being pure, was readily condensed. The temperature was somewhat raised. The silex precipitated, formed a gelatinous mass of a blueish colour, which had absorbed all the water like a sponge, so that none appeared fluid. This gelatinous mass was carefully transferred to a filter, and washed with distilled water till it was rendered insipid and incapable of reddening litmus paper. It retained its blueish hue only whilst moist. When dried and ignited, it was in thin lamellæ, and of a snow-white colour, and surprisingly bulky. It weighed 7·33 grains, and was found to be pure silex. Thus it appears that the subsilicated fluoric acid formed by the decomposition of 44 cubic inches of silicated fluoric acid gas contains 7·33 grains of silex less than the gas itself. Consequently, independent of water, which no doubt is essential to this acid, 100 parts of it seem to consist of

has lost more than one fourth of its silex by uniting with water.

54·56 silex
45·44 acid

100·00

Water condenses 263 times its bulk of sil. fl. a. gas;

I have endeavoured to ascertain what quantity of silicated fluoric acid gas a given quantity of water will condense. In one instance $\frac{19}{100}$ of a cubic inch of distilled water absorbed 51 cubic inches, barom. 30·5, therm. 60. The gas was added to the water in a jar over mercury, as fast as it was absorbed. The experiment was stopped, when the gas, after having remained in contact with the water a whole night, ceased to be diminished. According to this result, the proper correction being made for the additional pressure, water decomposes about 263 times its bulk of silicated fluoric acid gas.

or more correctly 365 times.

Dr. Priestley observed, that muriatic acid gas re-produced silicated fluoric gas from the crust of silex formed, when the latter is condensed by water*. This experiment I have repeated, and as it appears to show more correctly the quantity of gas water can condense, I shall describe the result. 2·4 cubic inches of muriatic gas were added to a drop of water, that had previously absorbed one cubic inch of silicated fluoric gas, in a jar over mercury. There was an immediate absorption equal to $\frac{2}{15}$ of a cubic inch. The mixture of silex and subsilicated fluoric acid effervesced, and from an apparent solid became fluid, the whole of the silex gradually disappearing. After the first mentioned absorption, there was no farther. The gas produced was silicated, as appeared from the crust it deposited when removed to water, and the liquid formed was pure muriatic acid, for decomposed by concentrated sulphuric, it afforded merely muriatic acid gas, without any silicated fluoric. The evident conclusion from the preceding result is, that water condenses equal quantities of the muriatic and silicated fluoric acid gases, and consequently that the first estimate is too low, and instead of 263 times its bulk, it is probably more correct to say that water to be saturated requires at least 365 times its volume. Neither will this estimate appear inconsistent with the former results, when the deposition of silex is considered as an obstacle to the free exposure of the surface of the water to the gas.

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* Vide Priestley on Air, Vol. II, p. 202.

Subsilicated fluoric acid is decomposed by alkalies, and by earths and acids.

Subsilicated fluoric acid is decomposed by ammonia and the fixed alkalies, and by all the earths that I have made trial of. It is also decomposed by the sulphuric acid and the boracic, as well as by the muriatic acid gas.

Of the particular changes which occur when it is acted upon by the alkalies, I defer giving any account at present, as it is my intention to do it in the next section.

Habitudes of subsilicated fluoric acid with heat. It came over in distillation, &c.

To learn the effect of heat on it, a small quantity of strong acid, pure and transparent, was introduced into a retort connected with mercury. A spirit lamp being applied, about three cubic inches of silicated fluoric acid gas were produced. The neck of the retort was lined with silex in a gelatinous state, and much liquid subsilicated fluoric acid, that had distilled over, was condensed in the colder part of the neck, and was absorbed by bibulous paper previously introduced, to prevent the distilled fluid from entering the jar for the reception of the gas. When the whole of the acid in the bulb of the retort had been evaporated, little or no silex remained.

This differs from Doctor Priestley's result.

The general result of this experiment is very different from that which Dr. Priestley, who first made it, obtained. Instead of silicated fluoric acid gas, he procured "vitriolic acid air," sulphureous acid gas.

I have tried also the effect of heat on the silicious crust, formed by the decomposition of silicated fluoric acid gas, by water; but could obtain no sulphureous acid gas, as Dr. Priestley did only a small quantity of silicated fluoric.

The correctness of Dr. Priestley's observations cannot be doubted. I can only account for his results, by supposing that some sulphuric acid in consequence of the high temperature employed in making the gas was volatilized, and mixed with the subsilicated fluoric acid, and that mercury also was present, from the acid being prepared over this metal.

Fluoric acid gas cannot be had by distillation free from silex.

These experiments too oppose another statement relative to a method prescribed for making fluoric acid gas free from silex, by merely heating strong subsilicated fluoric acid in a retort, and collecting the gas over mercury. It is asserted, in chemical works of some reputation, that this process is successful. I have never found it so, having always obtained results similar to those above stated. This, I suppose, is one of the many errors that have secretly crept into repute, and has been believed, because never subjected to the test of experiment.

The action of concentrated sulphuric acid on subsilicated fluoric acid, is similar to that of muriatic acid gas, occasioning a disengagement of silicated fluoric acid gas. Facts which appear to prove, that water is absolutely essential to the existence of this acid.

Sulph. acid expels silicated fluoric acid gas from subsilicated fluoric acid.

Boracic acid decomposes it, in a very different way, not from any predominant affinity for the water, but in consequence of a stronger attraction for the fluoric acid itself. Silicated fluoric acid of course is not produced; but liquid fluoboracic acid and the silex is precipitated in a gelatinous state, as when ammonia is employed.

Boracic acid unites with the fluoric acid, and both acids are precipitated along with the silex.

These are the principal facts I have to notice respecting this acid. Before I conclude, I shall briefly mention a few other circumstances. Applied to the tongue, in its concentrated state, it produces a very painful sensation, like that which strong muriatic acid does, and it has a very similar effect on the cuticle. It does not appear to erode glass, for I have kept it in bottles of this substance more than a month without any action being perceptible. Exposed to the air, it slowly and almost completely evaporates, there being only a very trifling silicious residue; and when gently heated in an open vessel, it is rapidly dissipated in white fumes.

Subsilicated fluoric acid acts on the tongue, and does not erode glass.

SECT. II. *On the Combinations of silicated fluoric acid Gas, and the subsilicated Fluoric, and the fluoric Acids with Ammonia.*

M. Gay Lussac has shewn that silicated fluoric acid gas, like carbonic acid gas, condenses twice its volume of the volatile alkali.* The experiment I have several times repeated, and constantly with the same result, no difference appearing when the acid gas was added in great excess to the alkaline, or the alkaline to the acid. This being the case, and knowing the specific gravity of the two gases,† 100 parts by weight of silicated fluat of ammonia seem to consist of

Silicated fluoric acid gas condenses twice its volume, or about one third of its weight of ammonia.

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* Vide Mém. d'Arcueil, Tom. II.

† According to Sir H. Davy, 100 cubic inches of ammonia, barom. 30, therm. 60, weigh 18 grains. It is this estimate which I have taken.

24·5 ammonia
75·5 acid
―――
100·0

Silicated fluat of ammonia volatilizes unaltered, if heated by a spirit-lamp in the vessel in which it is formed, and provided moisture be entirely excluded.

Water decomposes the compound, and precipitates silex.

Like silicated fluoric acid gas itself, this salt is decomposed by water, and a similar precipitation of silex occurs, and in the same proportion. Thus the salt formed by the union of 30 cubic inches of silicated fluoric gas, and 60 of volatile alkali (barom. 30°, therm. 60) in a small glass jar over mercury, being carefully collected and introduced into water, afforded five grains of pure silex, weighed after being well washed and heated to redness.

The aqueous solution is subsilicated fluat of silex and ammonia.

The saline solution, since part of the silex of the silicated fluoric acid gas is separated during its production, appears to be a subsilicated fluat, or a combination of subsilicated fluoric acid and ammonia. Another mode of making it, more directly proves that this is its composition. When ammonia is added to the subsilicated fluoric acid in excess, this salt is formed without any precipitation. From these facts, it may be concluded, that independent of water, which appears to be essential to its existence, 100 parts of it consist of

28·34 ammonia
71·66 acid
―――
100·00

It has a pungent taste; reddens litmus; crystallizes not deliquescent; corrodes glass while hot, &c.

Subsilicated fluat of ammonia has a pungent saline taste. It just perceptibly reddens litmus paper. Slowly evaporated, it forms small transparent and brilliant crystals. The largest I could obtain, appeared to be tetrahedral prisms. The solid salt is very soluble in water; but is not deliquescent. When heated it appears to sublime unaltered. It is curious that the solution of this salt, when evaporated by a heat near its boiling point, powerfully erodes the glass or porcelain vessel, and a residuum of silex appears, on the addition of water, to re-dissolve the salt. This erosion and residue of silex I have seen produced three times following, with the same quantity of salt. I mention the fact, which, I believe, was before observed by Scheele, without attempting an explanation of it. It may, perhaps be said, that as the water evaporates, the affinity of the subsilicated fluat for silex increases.

Is decomposed by sulphuric acids and alkalies.

Subsilicated fluat of ammonia is decomposed by the sulphuric acid, and by muriatic acid gas, and also by the fixed alkalies and by ammonia.

Effects of sulphuric acid,

Sulphuric acid expels from it, silicated fluoric gas and hydrated fluoric acid fumes.

and muriatic acid gas.

Muriatic acid gas acts slowly on it, and effects its decomposition apparently through the medium of its water. A little of the crystalline salt was introduced into muriatic acid gas in a jar over mercury. In a short time, some silicated gas was produced, as the silicious deposition, on the addition of water, indicated. Strong muriatic acid was substituted for the acid gas. Now no apparent change took place, for on evaporating the acid, the residue, decomposed by sulphuric acid, afforded only silicated fluoric acid gas.

Muriatic acid: no change:

Alkalies form the same salts as with the subsilicated acid.

The alkalies form by the decomposition of this salt, the same compounds that they do by their action on subsilicated fluoric acid.

Potash expels the ammonia, and produces the silicated fluat and fluat of potash, as MM. Gay Lussac and Thenard have described.

The changes occasioned by soda appeared to me similar: but the gentlemen just mentioned, assert that this alkali precipitates the whole of the silex, and does not form a triple salt with it and part of the acid.

Ammonia completely separates the silex.

Ammonia seems to me to separate completely the silex, and by uniting with the pure acid to constitute a true fluat. MM. Gay Lussac and Thenard are of a different opinion. They say that the whole of the silex cannot by this method be removed, but only the principal part. Their reason for this belief, is, that on repeatedly evaporating the salt after the addition of ammonia and re-dissolving it, they have each time observed a residue of silex. If they employed metallic evaporating vessels, the results of my experiments do not agree with theirs; for making use of platina for this purpose, and adding an excess of ammonia, I never detected traces of silex on evaporating the filtered fluat. But our results agree, if they employed glass or porcelain vessels, which fluat of ammonia has the property of corroding.

(To be Continued)

II.

Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut. Col. William Mudge. By Don JOSEPH RODRIGUEZ. *From the Philosophical Transactions for* 1812. *p.* 321.

(Concluded from p. 334, *Vol. XXXIII.)*

The uncertainty which may subsist respecting the figure, &c. of the earth, will affect the entire arc proportionally more than any part of the same; but the contrary happens in the English measurement:

But to return to our subject of the English measurement. If the uncertainty which yet subsists, with respect to the exact figure of the earth and its dimensions, occasions some small errors in the calculation of the series of triangles, the sum of these errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured. Now, in the English measurement, we find exactly the reverse of this. For the difference between the results of calculation and observation is only 1″,38 on the whole arc; but is even as high as 4″,77 on one of the smaller arcs. So that, whatever error we may suppose to have been introduced into the calculation, by assuming a false estimate of the spheroidity of the earth, or of other elements employed in the calculation, it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error, wholly independent of these elements.

which shews a considerable error of another kind in the obs

It was not till the date of the measurement of the meridian in France, that M. Delambre published and explained, with admirable perspicuity and elegance, all the formulæ and methods relative to the calculation of spheroids, and put it in the power of astronomers in general to make use of the elliptic elements in verifying the results of their observations. In the present state of science these elements are well known, and the errors that can arise from any uncertainty in them, are not so considerable as is generally supposed. The oblateness and the diameter at the equator are the only elements wanting in the calculation; for the purpose of seeing what effect our

The errors from uncertainty in the elliptic elements are not considerable.

our present uncertainty respecting them can have on the subject in question, I have employed three different estimates of the oblateness $\frac{1}{330}$, $\frac{1}{320}$, and $\frac{1}{310}$. With respect to the radius of the equator, that is ascertained with sufficient precision by the mean of the arc extended from Greenwich to Formentera, corresponding to latitude 45° 4′ 18″. The value of the degree in toises is 57010,5, and it is highly probable that in this estimate the error does not amount to so much as half a toise, as it is deduced from an entire arc of 12° 48′ between the two extremities, the latitudes of which have been determined with extreme care, and by a great number of observations.

By assuming three different estimates of oblateness the differences in the results—

The following are the logarithms of radius at the equator, which I have employed as adapted to each degree of oblateness, and opposite to them are placed the corresponding computed estimate of the entire arc between Clifton and Dunnose.

$\frac{1}{33}$ .... 6,5147,400...... 2° 50′ 21,972
$\frac{1}{320}$ .... 6,5147,485...... 2° 50′ 21,974
$\frac{1}{310}$.... 6,5147,570...... 2° 50′ 21,976

so that the greatest difference is but 0″,38 Let us suppose it 0′,4, or even 0″,5, for the second calculation was made only by means of the western series of triangles, and the third only was the eastern; but even then the error arising from uncertainty in the elements is not half the difference we find between the results of computation and of observations of the fixed stars. It appears, therefore, that these elements are by no means to be neglected as a method of verification; and in fact the quantity of 1″,38 is so small, that it is extremely difficult to ascertain this quantity with the very best instruments Of this we shall find further proof hereafter; but as this discussion is not without its use, I shall enter into some details on this subject.

prove too small to be in general ascertained.

The measurement in Lapland was performed by means of a double metre, and with a repeating circle of Borda, sent by the National Institute of France. In order to see to what degree of accuracy the arc computed would agree with that obtained by observations of the pole star above and below the pole, I assumed an oblateness of $\frac{1}{320}$ and as logarithm of adius I had 6,5147500 expressed in toises and in round numbers.

The same shewn from the Swedish observations;

bers. With these elements, and with the data to be found in the work of M. Svanberg, we have by the western series of triangles 5840″,196 and 5840″,138 by the eastern. So that the mean calculated arc is 1° 37′ 20″, 167, while the arc observed was 1° 37′ 19″,566. The difference then is 0″,6 for the total arc, and 0″,37 for the mean degree, or 5,86 toises excess in the linear extent. One can never depend upon quantities so small as this, so that the agreement between the results of computation and actual observation, proves not only the skill of the observers and the accuracy of which their instruments admit; but also that the elliptic elements employed in the calculation are a sufficiently near approximation to the truth to be deserving of confidence.

and also from the measures on the meridian taken in India by Major William Lambton;

In the 8th volume of the Asiatic Researches, published by the Society at Calcutta, are contained the details of another measurement performed in 1802, by Major William Lambton in Bengal, on the Coromandel coast. In this undertaking, which was executed with great skill and attention, Major Lambton employed Bengal lights as signals, chains for the linear measures, and a theodolite, and a zenith-sector made by Ramsden. The base measured was 6667,740 fathoms reduced to the level of the sea, and to the temperature of 62° Fahrenheit; and the stations were so chosen, that four of the sides of the triangles were almost in the same line, and nearly parallel to the meridian at the southern extremity of the arc, so that their sum but little exceeds its whole extent. The lengths of these arcs in fathoms reduced to the meridian are thus given in the Memoir of Major Lambton.

AB 20758,13 north latitude of A 11° 44′ 52″,59
BC 17481,245
CD 22237,04 north latitude of E 13° 19′ 49″,018
DE 35246,43

From these data Major Lambton deduces the degree of the meridian to be 60435 fathoms, or 56762,3 toises. By applying to this the same elements as we did to the measurement by Svanberg, we have the entire arc measured equal to 1° 34′ 55″,896; so that the difference between the results of calculation and of the observations is only 0″,532 for the whole arc, or 0″,337 for the mean degree. The elliptic hypothesis and observation agree more correctly in this instance, for the difference is rather less than in that of Lapland, although the two arcs are very nearly of the same extent. Thus the degree on the meridian measured in Bengal, in the latitude of 12° 32′ 21″ north, cannot be supposed to exceed Major Lambton's estimate by more than 5,22 toises; and it is extremely difficult to speak with certainty to quantities so small as this.

and also from a degree measured by Major Lambton, perpendicular to the meridian.

The same observer also measured one degree perpendicular to the meridian, by means of a large side of one of his triangles cutting the meridian nearly at right angles, and of which he observed the azimuth at the two extremities. The data from which his results may be verified are these:

Length of the chord of the long side in English feet AB= 291197,20.

Azimuth of the eastern extremity A equal to 87° 0′ 7″,54 NW.

Azimuth of the western extremity B equal to 267° 10′ 44″,07 NW.

North latitude of A 12° 32′ 12″,27
North latitude of B 12° 34′ 38″,86.

With these data in the triangle formed by the long side, the meridian at B, and the perpendicular from B on the meridian at A, we have the chord of this last arc equal to 290845,8 feet, and the arc itself 290848,03 feet. By applying the method of M. Delambre, we find the azimuth of the extremity B less by 2″ than it was observed to be; so that we have no reason to suppose a greater error than one second in the observation of each azimuth, and it seems next to impossible to arrive at a greater exactness.

The difference of longitude between the points A and B is 48′ 57″,36. With this angle and the co-latitude at A, we have in the spherical triangle right angled at the point A, the extent of the normal arc equal to 2867,330 seconds, and dividing its length in feet by this number, we have for the degree perpendicular to the meridian, at the extremity A, 60861,20 fathoms, or 57106,5 toises. Now these values are precisely what we find on the elliptic hypothesis, with an oblateness of $\frac{1}{340}$ or $\frac{1}{310}$; and in short, the correspondence between the hypothesis and the measures of Major Lambton, is as complete as can be wished. Major Lambton, indeed, finds the degree on the perpendicular too great by 200 fathoms, but this arises from a mistake in his calculation.

By applying the same method of computing the arcs in seconds and in toises, the elliptic hypothesis is found to agree with the late measures taken in France

Lastly, I shall apply the same method, and see how nearly the elliptic hypothesis agrees with the last measures taken in France, which merit the highest degree of confidence, both with respect to the observers who have executed it, and the means which they had it in their power to employ. I have taken only the arc between Dunkirk and the Pantheon at Paris, from the data published by the Chevalier Delambre in the 3d vol. of the Measurement of the Meridian. I employed the same elements and similar calculations to those made on the English arc. The oblateness of $\frac{1}{310}$ gives the difference between the parallels equal to 7883,615 seconds by the eastern series of triangles, and 7883,617 by the western series. The mean of these 7883,616 may be taken as the true extent of the total arc.

The two other elements give for this quantity, 7883″,621 and 7883″,403, or 2° 11′ 23″,6 and 23″,49, as the calculated extent of the arc. But the arc observed was 2° 11′ 19″,83, according to M. Delambre, and 2° 11′ 20″,85 according to M. Mechain; so that the least difference between the calculation and the observations will be 2″,64. M. Delambre is of opinion, that the latitude of Dunkirk, which is supposed to be 51° 2′ 9″,20, should be diminished; and in fact the distance between the parallels of Dunkirk and Greenwich, which is 25241,9 toises, gives by the mean of the three assumed ellipticities 26′ 32″,3 for the difference of latitude. After deducting this quantity from 51° 28′ 40″, the supposed latitude of Greenwich, there remains 51° 2′ 7″,7 or 8″, for that of the tower at Dunkirk. If from this again we deduct the calculated arc 2° 11′ 23″,5, we have 48° 50′ 44″,5 for the latitude of the Pantheon, while, according to the observations of M. Delambre, it is 49″,37, or 48″,35 by those of M. Mechain. If various circumstances, with regard to unfavourable weather, and also others of a different kind connected with the revolution, and of which M. Delambre complains with much reason, have occasioned some uncertainty with respect to the observations at Dunkirk, still the numerous observations made at Paris, both by him and by M. Mechain at a more favourable season, and in times of perfect tranquillity, render the supposition of an error of 4 seconds in the latitude of the

Pantheon wholly inadmissible. It is, however, too true, that such errors are possible, and it is only by careful perseverance, and by repeated verification, that they are to be discovered and removed, as we have seen to be highly probable with respect to the station at Arbury Hill.

Instance of irregularity in observing the difference of latitude

But the same celebrated observer, M. Mechain, who handled instruments with great delicacy, and was possessed of peculiar talents for this species of observation, has given us an instance of singular irregularity in the observations made at Montjui and at Barcelona.

between Barcelona and Montjui:

ascribed to local attractions;

but the deviations are of a contrary nature.

The latitude of Montjui, determined by a very long and regular series of zenith distances, is full 3″24 less than that deduced from a similar series of observations made at Barcelona, with the very same instruments, and with equal care. Moreover, there is reason to think, from other observations, that the latitude of Barcelona (which is supposed to be 45″) ought to be diminished still one second, so that the difference between the observations at Montjui and at Barcelona, will probably amount to as much as 4″. Local attractions are supposed to have been the cause of this irregularity; but then the latitude, as deduced from observations made at Barcelona, should have been less than it appeared by those made at Montjui itself; for the deviation of the plumb-line (or of the spirit contained in a level) *could only* be occasioned by the little chain of land elevated to 120 or 130 toises, which passes to the north of Barcelona in a north-easterly direction. Now since the deviations arising from this source would be northward, the zenith distance of circumpolar stars would be augmented by that deviation, and consequently the latitude deduced therefrom would be diminished by just so much. But here the contrary occurs; for the latitude of Montjui deduced from the observations at Barcelona is 48″,23, whilst that obtained by direct observations at Montjui is only 45″. Hence it seems probable, that the cause of this irregularity must be sought elsewhere, and that it is not likely to be discovered without repeating over again the same observations.

Moreover it does not follow that the latitudes of two places are correct, because the declinations of the stars deduced from them correspond; for the deviations caused by local attractions, or from any other source, are made to disappear in correcting the declination, but remain uncorrected in the latitude of each.

It is more probable that the error on the English arc may be in the observations.

Lieut. Col. Mudge is also of opinion, that the irregularity in the value of his degree may be ascribed to deviation of the plumb-line, occasioned by local attractions. This is certainly very possible, and may be decided by an examination of all circumstances on the spot. But if there be really an error of 1″ in the extent of the whole arc, this should rather be ascribed to some defect in the observations themselves, than to any extraneous source; for the observations of different stars give results that differ more than 4 seconds from each other.

Measurements still wanting in the southern hemisphere.

I shall now conclude this Memoir, by expressing a wish, which men of science in England have it more in their power than any others to gratify; I mean by making new measurements in the southern hemisphere. Those which have been made hitherto in the northern hemisphere are extremely satisfactory by their agreement, and give us great reason to presume that the general level of the earth's surface is elliptical, and very regularly so; and hence we might expect the opposite hemisphere to be equally so, and to be a portion of the same curve.

For the measures of Lacaille seem to indicate an ellipse of less eccentricity.

Nevertheless, the degree measured at the Cape of Good Hope by Lacaille, in latitude 33° 18′ appears to indicate an ellipse of less eccentricity, or of greater axis; for the linear extent of 57037 toises, corresponds to the measure of a degree in latitude 47° 47′ in the northern hemisphere. If now we calculate the arc as before, with an oblateness of $\frac{1}{320}$, and with the sides of Lacaille's triangles reduced to the meridian, we find it greater by 10″ than it was found to be by observations of the stars. An error of 10 seconds, by an astronomer so skilful and scrupulous as Lacaille, is too extraordinary to be admitted as probable. It is true, that there was a greater error well ascertained to have occurred in the measurement in Lapland, amounting to 13 seconds; but the academicians engaged in this undertaking were by no means equally conversant with observations as Lacaille.

Proposed admeasurement at the Cape,

There remains, therefore, but one method of removing all doubt on this subject, and this is to repeat and verify the measurement at the Cape, and, if possible, to extend it still farther to the north. The same Major Lambton, who has succeeded so well in Asia, and is in possession of such perfect instruments for the purpose, would be singularly qualified for a similar undertaking in Africa, and would furnish us with a measurement in the other hemisphere, as much to be relied upon as the former. He would have the glory of deciding two important questions by his own observations; first, the similarity and magnitude of the two hemispheres: and, secondly, the degree of reliance to be placed on the elliptic hypothesis.

and in New Holland.

It might be still further desirable, if other measurements could also be undertaken, either in New Holland, or in Brazil; for though neither of these countries differs much in latitude from the Cape of Good Hope, they are so remote in longitude, that a correspondence of measures so taken would nearly establish the similarity of all meridians.

Note.

Formulæ employed in the preceding computation.

I shall now explain the formulæ employed in deducing the results to which I have come in the foregoing Memoir. The demonstration of them is to be found in the work of M. Delambre, on the Meridian.

In the first place, let a be the radius of the equator, e the eccentricity, ψ the latitude of one extremity of a side, or arc, in any series of triangles, and θ the azimuth of that side. The radius of curvature of this arc will be expressed by

$$\frac{1}{R1} = \frac{\left(1 + \frac{e^2}{1-e^2} \cdot \cos.^2\psi \,.\, \cos.^2\theta\right)}{R} \text{ and } \frac{1}{R} = \frac{(1-e^2 \,.\, \sin.^2\psi)^{\frac{1}{2}}}{a}.$$

Hence we see that R is the radius of the arc at right angles to the meridian. One may in general neglect the azimuth, and take the last radius for the radius R1. Now, in computing the arc between Clifton and Dunnose, I have supposed the oblateness to be $\frac{1}{330}$ or $e^2 = \frac{669}{330^2}$, and log. $a = 6{,}5147200$ expressed in toises.

The latitude of the southern extremity of the base is the same as that of Clifton, and its azimuth, if we choose to attend to it, is nearly 335° 23′. This base, considered as an arc of a circle, is reduced to its sine by the formula $s = \log. s - \frac{K \,.\, s^2}{6R^2}$, Formulæ employed in the preceding computation.

(K being the modules of the table of logarithms, so that log. K=9,6377843.)

By means of the logarithmic sine of the base, and the angles of the triangles, considered as spherical, the logarithmic sines of the sides in the series were next computed, and then reduced to logarithms of the arcs themselves by the formula $\log.\ \varepsilon = \log.\ \sin.\ \varepsilon + \frac{K.\ \sin.\ ^2\varepsilon}{6R^2}$

For the purpose of making this last reduction, it is sufficient to take a single value of R, corresponding to the mean latitude of the entire arc 52° 2′ 20″. It was thus that the table was formed of logarithmic sides considered as arcs.

Let m be one of these arcs, and let us represent by $\delta\psi$ and $\delta\psi''$ its value reduced to the meridian, the one in toises, the other in seconds of a degree, and we shall have the following formulæ;

$$\delta\psi = m.\cos.\theta - \left(\frac{m^2.\sin,\ ^2\theta}{2R}\right).\text{tang}.\psi - \left(\frac{m^2.\sin.\ ^2\theta}{2R}\right).\left(\frac{m.\cos.\theta}{3R}\right).(1+3\tan.\ ^2\psi)$$

$$\delta\psi'' = \left(\frac{\delta\psi}{R.\sin.\ 1''}\right) + \left(\frac{\delta\psi}{R.\sin,\ 1'}\right).e^2.(1+e^2).\cos.\ ^2\psi.\left\{1 \mp \left(\frac{3\tan.\psi}{2}\right).\left(\frac{^2\delta\psi}{R}\right)\right\}$$

: the superior sign being taken when the latitude ψ'' is greater than ψ, and the inferior when it is less.

The correction dependent on the convergence of the meridian for the azimuths is $\delta\theta = \left(\frac{m.\ \sin.\ \theta}{R1.\ \sin.\ 1''}\right).\left(\frac{\sin.\ ^2(\psi+\psi)}{\cos.\psi'.\cos.\frac{1}{2}\delta\psi''}\right)$.

Hence the azimuth of the first station seen from the second and reckoned westward from the north, is $\theta' = 180^\circ + \theta + \delta\theta$.

If P″ be put for the difference of longitude between two points distant by an arc which measures m, we have $\sin.\ P = \frac{\sin.\ m.\ \sin.\ \theta}{\cos.\ \psi}$, $\log.\sin.\ m = \log.\left(\frac{m}{R_1}\right) - \frac{K}{6}.\left(\frac{m}{R1}\right)^2$, and $\log.\ P' = \log.\left(\frac{\sin\ P''}{\sin\ 1''}\right) + \frac{6}{K}.(\sin.\ P'.)$

The arc of the meridian, between Greenwich and Formentera, is so fortunately situated, that its middle point is in latitude 45°. Its whole extent measures 12° 48′ 44″, and the distance between the parallels, in linear measure, was found to be 730430,7 toises. Hence the mean degree, corresponding to the latitude of 45° 4′ 18″, is 57010,5 toises; and if we multiply

Formulæ employed in the preceding computation.

ply this number by 90°, we get one-fourth part of the meridian of the earth.

The correction to be deduced for oblateness is 58, 59, or 61 toises, according as it is assumed to be $\frac{1}{330}$, $\frac{1}{320}$, or $\frac{1}{310}$, and if we take the mean of these, we have the fourth part of the meridian Q = 5130886 toises; and hence the metre = 44330867 lines; so that the value of the metre turns out to be almost entirely independent of the elliptical form of the earth.

The radius of the equator is derived from the expression $\log. a = \log. \left(\frac{2Q}{\pi}\right) + K . (\frac{1}{2} . \varepsilon + \frac{1}{16} . \varepsilon^2 - \frac{1}{48} . \varepsilon^3)$, ε being the oblateness, and π the periphery of a circle = 3,1416.

In order to compare any degrees measured with those obtained on the elliptic hypothesis, we have a very simple formula. Let m and m' be the values of two degrees on the meridian, of which the mean latitudes are $\psi 1$ and $\psi 2$; in comparing the analytic expressions for these two degrees developing them, and then making $\psi = 45°$, we have $m' = m . (1 - \frac{1}{2} . p . \cos. 2\psi 2 + g . \cos.^2 2\psi 2)$, $m = 57010,5$ toises, $p = \frac{3}{2} e^2 . (1 + \frac{1}{2} e^2) . \frac{\sin. 1°}{1° . \sin. 1''}$, and $g = \frac{15}{64} . e^4 . \left(\frac{\sin. 2°}{1° . \sin. 1''}\right)$

And then we shall find that the oblateness $\frac{1}{320}$ gives 57075,66 and 57192,38 toises for the degrees in England and Lapland.

I shall here subjoin one reflection more, which appears of importance. The oblateness of the earth is a quantity which varies considerably, by the least difference in the elements on which it depends. Accordingly, it is not surprising, that its value fluctuates between two proportions which differ sensibly from each other. To illustrate this, let p be the function which serves to determine the oblateness of the earth, so that $\frac{1}{\varepsilon} = p$. When this equation varies $- \delta\varepsilon = \varepsilon^2 . \delta p$.

Now the coefficient ε^2 being very great, we see why the least variation in the elements of the function p, occasions so considerable a variation in the denominator of the oblateness. This is precisely what happens in the lunar equations dependent on the figure of the earth, and which M. Laplace has deduced from his beautiful theory. Thus, for example, in the inequality that depends on the longitude of the moon's node, which he has determined analytically with so much precision, the numerical coefficient found by Burg gives $\frac{1}{305}$ for the oblateness; but if this coefficient be diminished by 0″,665, then the oblateness becomes $\frac{1}{320}$, so that a variation even to this small amount in the coefficient augments the denominator of the oblateness nearly $\frac{1}{20}$ part.

The same happens with regard to the pendulum vibrating seconds; for, supposing its length at 45° to have been correctly ascertained by MM. Biot and Mathieu, if we wish to know the length of a second's pendulum at the equator, corresponding to an oblateness of $\frac{1}{320}$, we find it to be 439,1810 lines. Now this length differs from that determined by Bouguer only by 0,029 of a line, and M. Laplace even thinks that the result of Bouguer should be diminished by about double this quantity. We see from hence how much these little differences, whether produced by errors of observation, or irregularities in the earth itself, are liable to affect the denominator of the fraction expressing the oblateness.

Fortunately, it seems probable, that the utmost latitude of our present uncertainty is between the limits of 330 and 310, and the mean of these may be considered as a very near approximation to the truth.

III.

Critical Observations, on Dr. Wollaston's stated improvement of the Camera Obscura and Microscope in the application of the Meniscus, and Two Plano-Convex Lenses; proving their inferiority to the double Convex Lens generally used. By Mr. WILLIAM JONES, *Optician.*

To Mr. Nicholson.

SIR,

Reference to a former of the author upon periscopic spectacles.

AS my observations, published in your Journal, Volume 7, proved, I trust, that the Periscopic Spectacle Glass, advertised by Dr. Wollaston, as possessing a new optical principle,

ple, and affording an improvement in the figure of a spectacle glass, was no other than the old rejected Meniscus Lens; contained no principle of refraction, different from the plano-convex, and double convex lenses; but, as it caused a greater aberration of the rays of light than those two lenses, was a worse form of lens for spectacles, or any other instrument, than the double convex lens generally used by practical opticians: It must, therefore, surprise others, besides myself, that Dr. W. should be induced again to propose the Meniscus, as an improvement in the Camera Obscura, by substituting it for the double convex lens, his account of which you copied last month into your Journal, from the Philosophical Transactions for 1812.

Motive of the present memoir.

The desire I have to maintain an optical truth, and the duty I owe to our professional interest, obliges me again to point out to your readers, what I judge to be the error of his reasoning, and the fallacy of the inference.

Remedy stated by Dr. Wollaston as applicable to the common Camera; viz. to place the screen within the principal focus.

In his description of the effect of the double convex lens in the common camera, page 27, he states the known effect of the images distant from the middle, or direct focus of the lens, being somewhat indistinct, on account of the plane of representation becoming, in distance, greater than the principal focus of the lens; and the oblique pencils of rays being refracted to a focus, rather shorter than the principal one. "On this account," he adds, "it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre." The aberrations of the lens add also to the indistinctness.

Objection, and description of the remedy in use; viz. to make the screen concave.

The collateral indistinctness in our portable chest Cameras, is but trivial and unimportant; and, in my opinion, the remedy, as above proposed, will be found by the artist to be worse than the defect, as the distinct and vivid central images will be vitiated, and the extreme images but little improved. The most perfect remedy is that which has been used by opticians in large cameras, for more than 50 years past, of placing a bottom board, or whitened table, with a concave surface, proportionated to the focal distance of the lens; which, corresponding very nearly to the focus of all oblique refracted rays, exhibits universally the images with the greatest brilliancy and distinctness.

The exact curve of the surface of this board or table should be that of a conic section, but the concave (spherical) one answers sufficiently well. It is necessary for the reader unskilled in optics to know, that what opticians name the axis of a lens, is that imaginary line that is supposed to pass through its centre, and is not subject to any refraction, and all other rays incident on the surface are refrangible, in proportion to the angle they make with this axis, those rays impinging nearest the centre of the lens, and with the least obliquity of position, are refracted with the most perfect images, or with the least aberration, in double convex, plano-convex, and meniscus lenses. The longitudinal aberration produces a focus short of the principal one, and the lateral aberration a confused lateral extension of the images, blended with prismatic colour. These aberrations increase directly with the diameter and thickness of the lens, and inversely with its focus. In lenses of large diameter, and short foci, these aberrations will, by experiment, be rendered very manifest, and which have been clearly demonstrated by that learned optician, Mr. Benjamin Martin, in his Elements of Optics, Dr. Smith, and others.

Dr. Wollaston's improvement, with the meniscus described by quotation.

The subsequent paragraph, page 27, describes Dr. Wollaston's proposed improvement: the substance, in his own words, is as follows. "The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed, that its concavity is presented to the object, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted. The advantage of this construction over the common camera obscura is such, that no one who makes the comparison can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens any oblique
cil of rays is brought to a focus at a distance less than that of
cipal focus. But in the construction above described, the

the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For, since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E f, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius; and hence are made to converge to a point so distant, that the image (at f) falls very nearly in the same plane with that of an object centrally placed."

Observations to shew that the meniscus is inferior in its effect to the double convex lens.

The radii of curvatures for a meniscus of 22 inches focus, being as two to one, is not essential. The theory of dioptrics shews, the greater the proportion, or the nearer that the radius of one side approaches to infinity, or a plano, the more perfect the lens will be. Dr. W. has not stated the diameter of the convex lens, but the reader must suppose it to be four inches, like that of the meniscus; nor has he told the reader what improvement would be produced, if he placed a similar circular opening, or limited aperture, also over the convex lens. I must, therefore, inform the reader, and he may himself prove it to be correct. The diameter or aperture of four inches is too great for a lens of 22 inches focus, either double convex, or meniscus lens, placed in a Camera Obscura, as it transmits too much light, and produces too much aberration for the most distinct representation of the images within the Camera. Dr. W., therefore, no doubt, was obliged to correct this palpable defect, by a curtailment of the area of his lens no less than *three-fourths* of the whole, and the lens would have been more like one applied by a skilful optician, if he had at first inserted a lens of about two inches diameter. The limited aperture, therefore, it is evident, advantageously excludes superfluous rays, but has nothing to do with the *determination* of their *direction*. Upon a fair comparison, the reader will not only doubt of the superiority of Dr. W.'s Camera, but be *convinced* of its absolute inferiority; for the double convex lens, under a

Observations to shew that the meniscus is inferior in its effect to the double convex lens.

same diameter and focus as the meniscus, has less spherical surface, and consequently less longitudinal and lateral aberration of the two. Let us now advert to the transformation of the convex lens to become a meniscus, with the same focus; by considering their figures in the diagrams, the reader will perceive, that as much as the upper surface of the convex has been incurvated for a meniscus, so much the more has the convexity of the under side been augmented, to retain the original focus. The oblique pencils of rays first entering the meniscus, or any part of its surface, are from the immutable law of refraction refracted from the axis of the lens, contrarywise to the first direction on the convex, and afterwards in their passage into air, by the increased inferior convexity, refracted back towards the axis proportionally more than by the under side of the double convex to be converged to the same focal distance; and all pencils of rays that impinge on the surface in an oblique direction to its axis, must be united the same as by the convex lens, at a focus somewhat shorter than the principal focus from direct rays. The meniscus lens, in refractive property, differs not from the double convex one. The above explanation is agreeable to all writers on optics, and to correct experiment. In this meniscus, it is not "IF the incidence," &c. but the incidence *always* is so oblique on the second surface, as to increase the convergence; and no kind of opening E whatever will change nature's laws of refraction, so as to elongate the focus, or to produce two different focuses in one lens; and his previous explanation of "occasioning all pencils to pass, as nearly as may be, at right angles to the surfaces of the lens," page 27, is an irrelevancy in optics, and is the error of reasoning that I imputed formerly to Dr. W. on his spectacle glass. It is the angle that the rays make with the *axis of the lens*, of whatever shape, that refraction is estimated from, as the science teaches us; not from the geometrical positions of pencils and surfaces. From the greater aberration that the meniscus possesses, the images formed by it will be less distinct, have less light, and be more distorted than by the double convex lens. It is from the extended lateral distortion, and bringing the meniscus nearer to the plane than its exact focus, that I can assign a cause how Dr. W. could have fallen into the error; had he placed the concave side

it would have been a better position, the images would have been more defined and enlightened; it was so applied in his spectacles, the convex side being next to the object: but in neither case will the images be so perfect and vivid, as by the double convex lens. The meniscus in a Camera is not a new application; several, some years back, were made for the purpose, but not preferred. I can refer to the machine now existing with one. I have caused two lenses to be ground, one a double convex, the other a meniscus, as Dr. W. directs, of the same diameter, nearly four inches, and focus twenty-two inches; which experimentally verify the correctness of my observations, and which any intelligent person may inspect, by application at our manufactory, 30, Holborn.

The meniscus in a Camera not new. Reference to experiment.

The following quotations may to some of your readers better corroborate the truth of my remarks.

Quotations respecting lenses.

"If the side were concave (of a plano) so that the lens became a meniscus, there is no proportion of the radii, or position of the lens, with regard to the radiant, but what will give the aberration greater than the plano convex in its best position; and, since this was first observed by opticians, the meniscus began to lose ground in the construction of optical instruments, and is now quite rejected." Martin's Elements of Optics, 1759, page 29.

An oblique pencil of rays has its focus a little nearer the l (double convex) than a direct pencil. Cor. fig. 2.

This prop. holds good in a concave lens, and also in a meniscus, as well as in a convex one. Emerson's Optics, page 124, prop. 24.

"When parallel rays fall upon the plane side of a plano-convex glass, the aberration of the extreme ray, which is $\frac{9}{2}$ of the thickness, is less than the like aberration caused by any meniscus glass whose concave side is exposed to the incident ray.

"When the said glasses have their convexities turned to the incident rays, the aberration of the extreme ray in the plano-convex, which is now but $\frac{7}{6}$ of its thickness, is less than the like aberration of any meniscus in this position."

The best of all double concave glasses has the semi-diameters of its first and second concavities as 1 to 6; and consequently, this is the best figure of a glass to help short-sighted persons, as the double convex, one of the like figure is the best for spectacles." Smith's Optics, Art. 661, 662. 665.

" For since a meniscus, unless the surfaces of it are parallel to one another, has the same effect either that a convex lens, or a concave one would have, all the cases of diverging or converging rays that are refracted by it, will be the same with those already explained in the instances of convex or concave lenses." Rutherford's Philos. vol. 1, page 286.

" A plano-convex glass, with its $\left\{\begin{matrix}\text{convex}\\ \text{plane}\end{matrix}\right\}$ side towards the incident parallel rays, has less aberration than any meniscus with its $\left\{\begin{matrix}\text{convex}\\ \text{concave}\end{matrix}\right\}$ side exposed to parallel rays. Whence it necessarily follows, that that meniscus is best, which approaches nearest in shape to a plano-convex lens." Harris (of the Mint) Optics, 1776, p. 67.

So sensible have some optical glass grinders been of the impracticability and insufficiency of the meniscus glasses of short foci for spectacles, that I have in my possession some plano-convex and plano-concave glasses actually fitted in the frames, and sold for the *new periscopic glasses*.

Observations on the periscopic microscope.

The sort of French angle of reduction that Dr. W. has given, to obtain geometrically but nearly the radii of meniscus for a given focus, will be useless to the workman, as he already knows, by a very short arithmetical operation, how to obtain exactly such radii in half a minute's time, or a tenth part of the time necessary to construct that problem by Gunter's sliding rule, the time would be still shorter.

The combination of using two glasses in ordinary simple microscopes, or hand magnifiers, to diminish the errors arising from the spherical figure of one glass, was known to Sir Isaac Newton, and successive opticians. That late excellent practical optician, Mr. Ramsden, by the combination in the best position of two plano glasses, with their convex sides to each other, applied eye-pieces to his instruments with great advantage, to read off divisions of his circles, and magnify the wires of his telescopes, with clear definition at the circumference of the field of view, the diameters of the glasses being no smaller than the aperture of the tube. The same principle has since been advantageously applied to large object lenses for the lucernal microscope, by the late Mr. G. Adams, and ourselves, where the diminution of light was of less consequence than indistinctness of the image In many cases the combination of two convex lenses answer very well : but the combining of two similar plano-convex lenses together, of superfluous diameter and thickness, and for the greatest defect or aberration in the worst position to each other; and afterwards to palliate it with a small aperture as shewn in figure 4, is such an anomaly or absurdity in opitics as not to require any serious comment on my part. I shall only appeal to the least experienced constructor of microscopes, whether he does not know, that the substitution of a double convex lens of the diameter only of Dr W.'s aperture, and of the same focus, would produce an image infinitely more perfect and vivid than the mutilated one proposed by Dr. W.

From these remarks I presume there will be nothing to apprehend from the attempt of Dr. W. to depreciate the excellence of the spectacles, Camera Obsuras, and Mocroscopes, as have been constructed by the most eminent Opticians of the day.

I am, Sir,
Your's, &c.
W. JONES.

Holborn, 16th Jan. 1813.

IV.

Rules for discovering new Improvements, exemplified in the art of thrashing and cleaning grain ; hulling rice ; warming rooms ; preventing ships from sinking, &c. By OLIVER EVANS, *of Philadelphia**.

Origin of Inventions.

NECESSITY is called the mother of inventions ; but upon inquiry we shall find that reason and experiment bring them forth ; for almost all inventions have been discovered by such steps as the following ; which may be taken as a

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* From the Appendix to his " Young Mill-wright and Miller's Guide" printed by subscription in Philadelphia, but very sarce in this country.

Rule

Rule for inventing I. Consider the theory and present practice of art II. and what in speculation would be the best plan of operating: III. how far and in what respects the present practice can be improved: IV. make experiments or Trials of the plans thus deduced from reasoning.

Step 1. Is to investigate the fundamental principles of the theory, and the progress of the art or manufacture we wish to improve.

II. To consider what is the best plan in theory that can be deduced from, or founded on, these principles, to produce the effect we desire.

III Consider whether the theory is already put in practice to the best advantage, and what are the imperfections or disadvantages of the common process of the art, and whether they can be evaded and the process improved; and what plans are most likely to succeed.

IV. Make experiments in practice to try any plans that the speculative reasonings may propose or lead to. Any ingenious artist, taking the foregoing steps, will probably be led to improvement in his own art; for we see by daily experience that every art may be improved. It will, however, be in vain to attempt improvements, unless the mind be freed from prejudice in favour of established plans.

Example. I. *Thrashing of grain.*

Example 1. Suppose we take the art of thrashing grain.

Then by the rule.

I. Principles of the art. To disengage the grain: by beating or by rubbing.

Step 1. What are the principles on which this art is founded? The grain is contained in a head on the top of the straw enclosed in a husk, or chaff, that requires a force to break the hull, and disengage it; which may be done either on the principle of beating or of rubbing.

II. Theory apply force to the heads only.

II. What is the best plan in theory for effecting this? As we find that it requires nearly equal force, and is all contained in the head, which is much less in quantity than the straw, theory directs the force to be regularly and uniformly applied to the head only, which will require but little power, seeing we can rub it out between our hands.

III. Present practice. Thrashing by men: treading by animals.

III. How is this theory put in practice; and what are the imperfections and disadvantages of the common process? the grain in the straw is laid on a plank floor, and beaten by men, with flails; or on the ground, and trod out by horses.

Disadvantages.

The disadvantages are.

1. The force is employed on the straw &c.

1st. The force is in both cases applied equally to the straw, as well as the head.

II. Much force is lost, being unnecessarily expended in beating the straw, yet many heads escape undone, because the force is so irregularly applied.

well as the head; II. and force lost.

III. In treading by horses the grain [illegible] well as the straw gets dirty

III. Cattle make grain dirty.

IV. Thrashing by men is both expensive and tedious. Now cannot improvements be made to overcome all these disadvantages? Such speculations have produced several.

IV. Hand Thrashing is expensive and slow

First, a machine on the principles of a coffee mill, which requires very little force to rub the grain out of the heads, which are separated from the straw, by means of a machine on the principle of a comb, cutting them off. A machine to reap the heads without the straw is wanted to complete this theory, (in countries where the straw itself is not an article of demand).

Machines already made. (a) A mill to rub the grain from the heads.

Secondly, a machine invented and put in practice by Colonel Alexander Anderson, of Philadelphia; the principles of which are to apply the strength of horses to strike the straw regularly with a uniform force, which finishes as it goes and clears the grain at the same time.

(b) Revolving Cylinder with beaters on its circumference, commonly used in England.

A Cylinder, 4 feet long, and 3 feet 6 inches diameter, with eight bats fastened to its circumference parallel to it axis, and of its whole length, is made to revolve with rapidity; the bats strike the straw at every fourth of an inch, it being drawn into the machine by and between two collars that move slowly. This machine makes great dispatch, but is expensive. (and destroys the straw.)

Others, attending to the principles of treading, have made a thing in the form of the frustrum of a cane or sugar loaf, set full of cogs to act like the horses' feet. This is drawn by horses round a circular floor, adapted to it, on which the grain is laid, the centre of the circle being the vertex of the cone. This having considerable weight and many cogs, a horse will beat out much more with it than with his feet, because it will strike a great many more strokes with equal force: it has these advantages; it can be made by an ordinary carpenter—is cheap—and the dirt is not mixed with the grain, straw, &c.

(c) a rolling cone with cogs or studs.

Example II. The art of cleaning grain by wind.

By the rule.

Example II *Art of winnowing.*

Step 1. what are the principles on which the art is founded Bodies falling through resisting mediums, their velocities are as

Its principle: Light bodies

are acted upon by the air more than heavy ones.

their specific gravities; consequently, the farther they fall the greater will be their distance; on this principle a separation can be effected.

Practical result. Blow the light bodies from the grain, by a current of air; deep; but not wide, across the stream. Let the grain &c. fall into compartments and use the same blast repeatedly.

II. What is the b[illegible] plan in theory? First make a current of air for the grain to fall through, as deep as possible. Then the lightest will be carried farthest and the separation be more complete at the end of the fall. Secondly, cause the grain, with the chaff, &c. to fall in a narrow line across the current, that the light parts may meet no obstruction from the heavy in being carried forward. Thirdly, fix a moveable board edgeways to separate between the good clean grain and light grain &c. Fourthly cause the same blast to blow the grain several times, and thereby effect a complete separation at one operation.

Present practice. The grain does not fall through a suitable cavity; nor is it cleaned at one operation.

III. Is this theory in practice already, and what are the disadvantages of the common process? We find that the common farmers' fans drop the grain in a line 15 inches wide, to fall through a current of air about 8 inches deep, (instead of falling in a line $\frac{1}{2}$ an inch wide through a current 3 feet deep) so that it requires a very strong blast even to blow out the chaff; but garlic, light grains, &c. cannot be got out, they meet with so much obstruction from the heavy grain. It has to undergo 2 or 3 operations; so that the practice is found to be no way equal to the theory; and appears absurd when tried by the scale of reason.

Plan of improvements.

IV. The fourth step is to construct a fan to put the theory in practice, to try the experiment*.

Exp. III. *Art of warming apartments.*

Example III. The art of warming rooms by fire.

Nature of fire not discussed.

Step I. The principles of fire are too mysterious to be investigated here; but the effects are,

Effects I It rarefies the air.

1st. The fire rarefies the air in the room, which gives the sensation of heat or warmth.

II. Causes part to ascend;

II. The warmest part being lightest, rises to the uppermost part of the room, and will ascend through holes, (if there be any) into the room above, making it warmer than the one in which the fire is.

III. particularly in the chimney, which pro-

III. If the chimney be warm, the air will fly up it first, leaving the room empty. The cold air will then rush in at all crevices to supply its place which keeps the room cold.

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* This Machinery, with a large passage or channel, is useful to clean feathers from dirt and heavy bodies.—W. N.

duces cold drafts.

II. Considering the principles, what is the best plan in theory for warming a room?

Practical result.

I Employ the whole heat in warming the air.

I. We must contrive the fire to spend all its heat to warm the air as it comes in the room.

II Prevent its escape, and ventilate by escape of cold air.

II. To retain the warm air in the rooms, and let the coldest out first to obtain a ventilation.

III Warm and ventilate several apartments

III. Make the fire in a lower room, conducting the heat through the floor into the upper one, and leaving another hole for the cold air to descend to the lower room.

IV. heat the air as it comes in.

IV. Make the room perfectly tight so as to admit no cold air, but all warmed as it comes in.

V. Limit the aperture of the chimney.

V. By stopping up the chimney to let no warm air escape up it, but what is absolutely necessary to kindle the fire, a hole of two square inches will be sufficient for a very large room.

VI Supply the combustion with air from without.

VI. The fire may be kindled by a current of air brought from without, not using any of the air already warmed. If this theory, which is founded on true principles and reason, be compared with common practice, the errors will appear the disadvantages of which may be evaded.

Experiment or trial.

III. I had a stove constructed to put the theory as fully in practice as possible, and have found all to answer according to theory.

A stove described in general terms

The operations and effects are as follows, viz.

I. It applies the fire to warm the air as it enters the room, and admits a full and fresh supply, rendering the room moderately warm throughout.

II. It effectually prevents the cold air from pressing in at the chinks or crevices, but causes a small current to pass outwards.

III. It conveys the cold air out of the room first:—consequently,

IV. It is a complete ventilator rendering the room healthy.

V. The fire may be supplied in very cold weather by a current of air from without, that does not communicate with the warm air in the room.

VI. Warm air may be retained in the room any length of time at pleasure; circulating through the stove, the coldest entering first to be warmed over again.

VII. It will bake, roast, and boil, equally well with the common tin-plate stove, as it has a capacious oven.

VIII. In consequence of these philosophical improvements it requires not more than half the usual quantity of fuel*.

Ex. 4 Art of hulling rice.

Example IV. The art of hulling and cleaning rice.

Principle. The outer coat of the grain is sharp, and the surfaces if rubbed together, cut each other.

Step 1. The principles on which this art may be founded, will appear by taking a handful of rough rice, and rubbing it hard between the hands; the huls will be brushed off, and by continuing the operation, the sharp texture of the outside of the hull (which through a magnifying glass appears like a sharp fine file, and no doubt is designed by nature for the purpose) will cut off the inside hull; the chaff being blown out, will leave the rice perfectly clean, without breaking any of the grains.

II. What is the best plan in theory for effecting this*?

Plan for practice.

III. The disadvantages of the old process are known to those who have it to do.

Art of preventing ships from sinking.

Example 5. To save ships from sinking at sea.

Principles. why bodies float stated in a popular way.

Step 1. The principles on which ships float, are the difference of their specific gravities, from that of the water, bulk for bulk, sinking only to displace water equal in weight to the ship; therefore, they sink deeper in fresh than in salt water. If we can calculate the cubic feet a ship displaces when empty, it will shew her weight, and subtracting that from what she displaces when loaded, will shew the weight of her loaded. Each cubic foot of fresh water being 62·5lbs. if an empty rum hogshead weigh 62·5lbs. and measure 62 cubic feet, it will require 875·lb. to sink it. A vessel of iron, &c. filled with air, so large as to make its whole bulk lighter than so much water, will float; but if the air be let out and filled with water it will sink. Hence, we may conclude, that ships loaded with any thing that will float will not sink, if filled with water; but, if loaded with any thing specifically heavier than water, will sink as soon as filled.

II. This appears to be a true theory.—How is it to be put in practice, in case a ship springs a leak that gains on the pumps†?

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* The discription will appear in a future number of our Journal.

† He describes a machine, which likewise deserves to be attended to, though less immediately connected with the industry of Great Britain. I shall consider it. W. N.

III. The mariner who understands well the above principles and theory, will be led to the following steps:

Practical result.

1st. To cast overboard such things as will not float, and carefully to reserve every thing that will float, for by them the ship may at last be buoyed up.

Throw the heaviest things overboard.

2nd. Empty every cask or thing that can be made water tight, and put them in the hold, and fasten them down under water, filling the vacancies between them with billets of wood, even the spars and mast may be cut up for this purpose in desperate cases, which will fill the hold with air and light matter, and as soon as the water inside is level with that outside, no more will enter: if every hogshead buoy up 875lbs. they will be a great help to sustain the ship, (but care must be taken not to put the empty casks too low, which would overset the ship) and she will float, although half her bottom be torn off. Mariners for want of this knowledge often leave their ships too soon, taking to their boat, although the ship is much the safest, and does not sink for a long time after being abandoned; not considering, although the water gain on their pumps at first, they may be able to hold away with it, when arisen to a certain height in the hold; because the velocity with which it will enter, will be in proportion to the square root of the difference between the level of water inside and out; added to this, the fuller the ship, the easier the pumps will work; therefore, they ought not to be so soon discouraged.

Empty the casks, and bung them well up.

Ships are safer than boats.

Pumps are easily worked in a water-logged ship; which may, therefore, be long kept afloat.

V.

Useful or Instructive Notions, respecting various objects. 1. Multiplying of Copies of Writing. 2. Scintillation of the Stars. 3. Large Achromatic Lenses.—W. N.

1. *Art of Copying, or of multiplying Copies.*

EVERY one is aware of the invaluable benefits which society has derived from the arts of printing, by moveable types, as well as by blocks and copper plates. But there are many cases, in which it would be of advantage to produce copies of writing, without requiring a stock of types or engraved plates; and the presses, or implements, by which the impression is made. A

Benefits of the art of printing.

saving, either in machinery, labour, or skill, is much to be desired. Under the present head, I have a few observations and facts to offer, relative to manuscript writing. The celebrated James Watt, about thirty years ago, obtained a patent for a copying machine, for making copies of the description known by the name of counterproofs. His apparatus, consisting of a portable rolling press, a receptacle for keeping very thin unsized paper in a due state of wetness, and a peculiar ink more mucilaginous and less speedy in drying than common writing ink, is at present in general use, particularly in merchants' counting houses. In a former Journal it was remarked, that sugar or treacle, added to ink, gives it the disposition to come off upon wet paper, and that if the paper be well soaked, so as not to shine and yet to be considerably transparent, a very light pressure, such as that of a warmed flat iron, would produce the copy.

James Watt's copying machine,

It is to be regretted, that this ingenious application shoul require as much apparatus and skill as it does; though its value is undoubtedly very great. The following process is less neat, but may be practised wherever a round ruler and gauze paper, or blotting paper, can be had. I have availed myself of it on a journey; in which it first occurred to me as an expedient for copying letters.

requires preparation and apparatus.

The process.—Roll a piece of guaze paper upon a small sound ruler, and place the ruler, thus covered, upon the sheet of paper intended to be written upon, in such a manner as that the ruler shall be just above, and parallel to the intended first line, and the outer edge of the gauze paper on the same side as the upper edge of the paper. Then write the first line, and immediately upon concluding the same, roll the ruler just upon it; and the gauze paper will receive a print of that line. Return the ruler to its first position, write a second line, and take a print of that as before.—And in this manner the whole letter may be copied while writing. I found a little awkwardness at first, in bringing myself into the habit of this manipulation; which requires the writer to recollect, at the end of every line, that he is to apply the gauze paper; but this was soon overcome. And it may also be observed, that for a very light hand, which dries quickly, it would probably be needful to apply the ruler at shorter intervals. My hand writing, which is neither heavy

Another process which can be practised in all situations.

heavy nor light, admitted of the operation being performed, as before directed, but I could not defer it to any second line.

Art of making copies by black tracing paper.

Another artist, of the name of Wedgewood, has, within a few years past, offered to the public, under sanction of Letters Patent, the engraver's method of tracing, by means of a piece of paper blacked with a pigment, (commonly lamp-black) applied by means of fat or a slowly drying oil. If such paper, which is sold at the shops, by the name of black tracing paper, be laid upon a leaf of common paper, and another leaf be laid upon that, the whole being disposed upon a firm flat table or plate of wood, or metal, or glass, and any writing be made with a small rounded steel or glass point, two copies will, by the same operation, be produced; viz. a reverse copy on the upper white paper, and a direct copy on the lower; the latter of which is sufficiently durable to be sent away to a correspondent, and the former will be very legible, as a direct copy, if the paper be thin.

A method of speedily writing upon, and printing from a metallic plate.

Dr. Franklin mentioned to the Abbé Rochon* a method of rapidly engraving or marking plates, for multiplying copies. He wrote with gummed ink, upon a surface of hard stone or iron, and powdered his writing with sand, or emery, or cast iron dust; and when dry, he applied another plate of soft wood, or pewter, or copper, upon the surface, and forced the gritty matter into this last by the action of a press. This last served, in the usual method of copper plate printing, to give a very great number of copies, not neat or beautiful, but sufficiently legible.

Another by etching and making counter-proofs.

The Abbé Rochon proposes, as a better method, to write with a steel point upon a copper plate ready varnished, and etch the face by aqua fortis. Reversed prints being taken from this etching, he piles these, while wet, along with other damped paper, and passes the whole through a press, which gives an equal number of counter proofs not reversed.

Improvements suggested.

Both the last mentioned methods may be of use in armies and under other circumstances: but both suppose extensive means and apparatus, and only dispense with the engraver's skill. Perhaps it would be an addition to Rochon's method, that the etching should be omitted, and the writing made upon soft metal with a sharp point leaving the bur on. Such a plate would afford many impressions.

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* Recueil de Memoires, &c. from M. L'Abbé Rochon, octavo, Paris, 1783, p. 343.

It would be a great improvement upon Watt's method, if the Counter-proofs could be taken upon dry paper. The tracing paper of Wedgwood and the engravers soon loses its colour, and it will not keep long. It soon becomes too dry to give off its colour.

2. *Scintillation of the Stars.*

Twinkling of the stars ascribed to the air.

Many speculations have been offered to account for and explain that apparently irregular and agitated emission of light, from the fixed Stars, which has been called scintillation or twinkling. From its marked appearance at low attitudes, and almost total absence at higher, it has been commonly ascribed to the interposed atmosphere; which, by the changeable densities of its parts, and the interposition of opake particles, is imagined to produce variations in the quantities, colours, and directions of the light before it arrives at the eye. In proof of this doctrine it has been farther noted, that the stars do not scintillate in a telescope. Undoubtedly the effect is still clouded with uncertainty. An observation I made upon the Dog Star (Sirius) in the autumn of 1807 may be considered as affording a few facts more in addition to those we already possess.

The stars do not twinkle in a telescope,

but give coloured rays in succession.

An experiment on the coloured rays of the Dog Star.

It is not true that the stars have no scintillation in a telescope. It may be strikingly observed by putting the instrument out of adjustment. In this case the circular disc of light, has a kind of vaccillation, as if a number of discs were continually flashing before each other: the illumination seemed to come on at different sides, and these discs also differ in colour. Blue, steel blue, pea-green, bright copper, red and white, are among the most usual colours; but the rapidity of succession does not allow the sense to determine whether these colours may be more or less cotemporaneous, or completely and distinctly succeeding each other. To determine this point, I took an achromatic glass of Ramsden's, magnifying 24 times, and directed it to the star—the object end being supported in a notch in a steady bar connected with the wall, and the eye end, upon an adjustable piece which was likewise capable of being not very steadily. But upon this I rested my left hand, between the finger and thumb of which I held the eye end of the glass. In this situation, the glass being truly adjusted to distinct vision, I could observe the star, and by gently and rapidly striking the tube with the fingers of the other hand, I caused the image of the star to dance in the field of view, and describe the same kind of luminous line as is seen when a lighted coal is whirled about. The star was thus made to describe by each blow a curve returning into itself; but so contorted and irregular that no two successive curves were coincident with each other. The strokes were about ten in a second of time, and the curves were beautifully and distinctly tinged with different colours in their successive parts thro' different lengths: but it seemed at a medium that each of these vivid colours might occupy about one-third part or less of the whole curve, and upon my recollection those most predominant were greenish blue, steel blue, and maroon or an intense copper colour. The light from Sirius therefore as it arrived at the eye was by extremely sudden variations distinctly changed in its colour, at least thirty times in one second. No theory deducible from the known properties of the atmosphere, as an interposed medium, has yet presented itself to my mind, in a shape worthy of notice.

The rays were beautifully distinct and vivid.

and varied thirty times in a second.

In the collection last quoted of Rochon, p. 380, he observes, that the scintillation of the fixed stars is an obstacle to measuring their diameters, and that when the light of Sirius was refracted into colours by a prism, it had no scintillation across the spectrum. As far as may relate to the apparent diameters of the fixed stars, the observations of Herschel do not seem to support the deduction of Rochon; but his fact appears to correspond with mine.

Correspondent fact with a prism.

3. Advantage of upsetting or pressing in the borders of plates of flint glass to make the concave lens in achromatic combinations.

The same Abbé Rochon p. 372, remarks that the triple object lenses of Dollond of 3½ inches aperture, produce an effect equal to that which it seems ought to be obtained from the lenses of 30 or 40 feet, made by Campani. But that in making achromatic lenses of longer focus, the plates of glass being blown, are too thin to be worked without bending and spoiling the figures. All the cast glass he tried was found to be

Large achromatic lenses

cannot be made for want of thick flint glass.

Blown glass is superior to cast.

be more unequal in its quality, in different parts of the same plate, than the blown glass. It would not be difficult to explain this from the circumstances of the making; but the principal object of the present notice is, to mention that he succeeded in making a thick lens out of plate one quarter of an inch in thickness, by softening the glass by heat upon an earthen mould of the proper curvature, and upsetting or pressing the borders inwards, (taking care to avoid folds or wrinkles,) till the edge was an inch thick, and the diameter five inches. He then surrounded the glass by a metallic ring of six inches diameter, and three quarters of an inch deep. Within this ring he again heated the glass, upon which he previously placed an upper convex earthen mould. The glass thus obtained appeared very good, and when ground and polished, enabled him to make a triple object glass of seven feet focus, producing, as he says, a much greater effect than the glasses of Dollond, but without admitting of a proportionate aperture. For the lenses of that celebrated artist bore an aperture of 42 lines, and his lenses would not admit of more than 4 inches or 48 lines; which, however, adds more than one third to the whole quantity of light. From the great care in working, he did not think that the external parts of the lens were defective on account of the figure. The defect arose most probably from the flexure and contortion of the grain of the glass in pressing in. For an ingenious philosophical artist has assured me, that there is great difference in lenses and prisms made of the clearest plate glass; accordingly as the line of vision is directed at right angles to the natural plane, or more obliquely or coincident with it, the latter being in general good for nothing. Whence, and from other facts, he inferred that the layers of glass plates differ considerably in their densities.

Blown plate glass may be worked up thicker.

A lens, made in this manner, was excellent;

but the borders defective, probably from the glass and not the figure.

VI.

An Account of some Experiments on the Congelation of Mercury, by means of Ether. By ALEXANDER MARCET, M. D. F.R.S.

To Mr. Nicholson.

SIR,

Account of Mr. Leslie's method of freezing.

MR. Leslie's new and ingenious mode of illustrating the well known fact of the production of cold by evaporation, by actually freezing water, in consequence of a rapid process of vaporization from the water itself, has already become a familar experiment. Water is placed over an open vessel, containing sulphuric acid, and the whole being inclosed within the receiver of an air pump, the water cools as the exhaustion proceeds, and is ultimately converted into ice. I have learnt also, that Mr. Leslie has succeeded in freezing mercury by a similar process; that is, by investing the bulb of a mercurial thermometer with a thin coat of ice, and exposing this to the joint effect of exhaustion and of sulphuric acid.

Mercury frozen by evaporation of ether.

After trying to repeat the last of these experiments, (an attempt in which I did not succeed) I effected the congelation of mercury with great facility and quickness, simply by substituting the evaporation of ether, instead of that of water, in the process in question. I am not aware of having been anticipated in this experiment; if I have, you will oblige me by taking no notice of this letter; but, in the contrary case, I shall thank you to give it a place in your Journal.

Method of making the experiment.

The mode in which the experiment is made is this: a conical receiver, open at the top, is placed on the plate of the air pump, and a mercurial thermometer is suspended within the receiver through the aperture. This is done, like some of the well known pneumatic experiments, by means of a brass plate perforated in its centre, and fitting the receiver air tight when laid upon its open neck. The thermometer passes through this plate to which it is carefully fitted by a leather adjustment, or simply by cork, secured with sealing wax; and it is so graduated, that when its bulb is sunk a few inches within the receiver, the stem rises externally through the plate, above which the scale begins. The bulb is then wrapped up in a little cotton wool, or what is better, in a little bag of fine fleecy hosiery; and after being dipped into ether, the apparatus is quickly laid over the receiver, which is exhausted as rapidly as possible. In two or three minutes the temperature sinks to about 45 below 0, at which moment the quicksilver in the stem suddenly descends with great rapidity, (in consequence of the remarkable contraction which the mercury in the bulb undergoes in congealing) to a distance corresponding to between 300 and 400 degrees. This, however, seldom happens to that extent, because the descent of the mercury is often impeded by the freezing of the column itself at the entrance of the bulb, before the congelation within the bulb is completed.

The facts particularly stated.

If it be desired to exhibit the mercury in its solid state, common tubes may be used, which should be broken instantly after being removed from the pump. I have frozen in this way bulbs of an elongated shape, about an inch in length, and near an inch in diameter. The pump I have used for these experiments is one of a small size;* the gage of which stands at about a quarter of an inch, when the exhaustion is pushed to its utmost extent. I have occasionally succeeded in this experiment, when the temperature of the room, as well as that of the ether, was about 50°; but the certainty of success is much increased by operating in a room, the temperature of which does not exceed 40°, and by previously reducing the temperature of the ether. I have been in the habit, in making this experiment, of inclosing sulphuric acid within the receiver, as in Mr. Leslie's process, as it has appeared to me to promote the evaporation of the ether, and the production of cold; but the experiment has also succeeded without the assistance of sulphuric acid.

Variation of the experiment.

The same experiment may be varied by first dipping the bulb of the thermometer, surrounded with cotton wool or flannel, into water, and after freezing this by means of the pump, pouring a few drops of ether upon the frozen bulb, and exposing it again to the effect of exhaustion. This plan has sometimes succeeded when circumstances were not sufficiently favourable for the success of the other.

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* Made by Mr. Bate, instrument maker in the Poultry.

Water frozen, by applying the air pump to Dr. Wollaston's instrument.

I have applied a method, similar to those just described, to the freezing of water, by means of the ingenious instrument imagined by Dr. Wollaston*, to which he has given the name of chryophorus. This instrument consists in a tube, terminated at each extremity by a ball, like the common pulse glass, one of these being full of water, and both the balls and tubes being completely exhausted of air. By plunging the empty ball into a mixture of salt and snow, the water in the other ball, though at some inches, or even some feet distance from the cold mixture, is frozen in a few minutes. But by a process, similar to that I have just described, for the congelation of mercury, the same may be effected without any cooling mixture in less than one minute, and with a pump of very moderate power. I may take this opportunity of mentioning, that having constructed an aparatus of this kind, with a thermometer within it, I observed that the temperature of the water sunk to 20° ; and, in one instance, even two or three degrees lower before it froze, which I at first ascribed to the water being deprived of its air by previous boiling ; but the same circumstance not having uniformly taken place, when the shape and size of the apparatus, and the quickness of the process, were varied, I am now inclined to ascribe it to other causes.

I have the honour to be, &c. &c. &c,

ALEXANDER MARCET.

Russell Square, 22*nd Jan.* 1813.

VII.

Observations upon the best state in which it is advisable to bring the British Merino Wools to market. By EDWARD SHEPPARD, Esq. of Uley, in Gloucestershire.

Introduction.

MR. SHEPPARD has made his title good to that fame which attends the patriotic and well-directed exertions of so many of our country gentlemen, in improving our valuable stock of first materials. Wool has, for centuries, been considered as one of the first, and Mr. S. has claimed and received the Gold Medal of the Society of Arts, for having produced from his flocks of 1929 Merino and Merino Ryland, the whole bred and kept by him, 7749 lbs. of wool in the year 1812. He has communicated the following observations to the society.

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* This apparatus was described a few weeks ago, by Dr. Wollaston, in a paper which was read before the Royal Society, an abstract of which was published in the 1st number of Dr. Thomson's Annals of Philosophy.

The Author's experience in the growth and manufacture of wool.

Having had the experience of more than ten years, both in the growth and manufacture of British Merino wools, which, by the constant use of the Spanish rams that came into his Majesty's possession during that period, I have brought to very great perfection; I take this method of making public the result of my observations, as to the mode most profitable for the grower and manufacturer, to prepare the Merino wools for the market; as considerable difference of opinion and practice prevail on the subject.

The superior softness of the Saxon and Anglo-Merino wool arises from their being kept in their grease.

I had the honour, in the year 1806, to present a memoir to the Board of Agriculture, in a successful claim I made, for the Gold Medal given for the greatest quantity of fine wool, grown within the year. I therein stated my opinion, that the principal cause of the superior and characteristic softness of the Saxon and Anglo-Merino wools, was, their remaining in their native grease, without its being expunged in the extreme degree practised in Spain. Excepting the moderate washing that Saxon and British wools receive on the sheeps' back before shearing, they continue in their grease till they are worked up by the manufacturer; while the wools in Spain, as soon as shorn, are thoroughly scowered, by an injudicious process, and then exposed for days to a burning sun, in which brittle and hard state they are so closely packed up, that they come out of their bags here, almost as much pressed and hard as hops, wholly deprived of that unctuous preservative, which I conceive to be necessary to the soft feel of wool.

The wools of Spain are harsh because too much scowered,

and would most probably be soft if better managed.

It has been thought by some, that Saxon and Anglo-Merino wools have a softness peculiar to themselves, and different from the Spanish, their parent stock, obtained from their cross with another and coarser woollen sheep. I am, however, very much disposed to attribute the quality here spoken of, to the better management of the wools in this country. Unfortunately, we have no opportunities of discovering what Spanish wool would be preserved in the grease; as the mode of laying on the duties at Burgos, by the pound, prevents the grower or merchant exporting it in that condition. Otherwise, I am much inclined to think the same softness would be found in the pure parent fleece, as in the spurious offspring. From the small experience afforded by the ill-conditioned fleeces lately imported with the sheep from Spain, I am very much confirmed in my opinion.

Lambs' wool of Spain is less cleansed, and is softer.

Lambs' wool, not being so completely washed from its grease in Spain as sheep's wool, comes very near to the softness of the Saxon and British lamb's wool. As a proof of their possessing an extra quantity of grease, they are much sooner liable to breed the worm than Spanish sheep's wool. I have often proved, in the manufacture of wool, that where it has been long saturated with oil, artificially, the fibre has been lubricated with it, and the cloth very superior in feel and softness.

Vell wool is very harsh because cleared of all grease,

It has long been known to manufacturers and wool-staplers, that the wool of dead sheep, or Vell-wool, as it is called, is very harsh, and quite unlike the same wool shorn from the sheeps' back, occasioned by its being disengaged from the skin, by the fell-monger, by the action of lime, which entirely dries up and destroys the oily particles. May it not, in some measure, arise from the cause, that wool from sheep used to calcareous or silicious soils, is of a harsher description; as those from the Sussex, or Wiltshire downs, when compared with the fleeces grown on the argillaceous lands of Hereford and Shropshire? The absorption of the native grease, by the frequent contact of the sheeps' coat with the soil, and the dust from it, may help to remove that great preservative of softness, and leave the fibre exposed, unprotected by moisture, to the action both of the sun and rain, which, in those exposed situations, would act with double power.

and this may be the cause of harsh wool on chalky soils.

Inferences: that the sheep should be protected.

From the above theory I would wish to deduce a few inferences, which may be of service in the growth and management of British fine wools. In the first place, I am satisfied that nothing can so much tend to preserve this necessary state and not often washed,

of native grease, as the protecting the fleece from the humidity and inclemencies of climate. In a country where such exist in any great degree, it would be requisite, in *order to attain and preserve a superior degree of fineness*, that the sheep be housed in the winter, as practised in Saxony, and the northern parts of Germany, where they not only cot them in the winter, but drive them under cover at every thunder-storm in summer. The frequent washing of a sheep's coat, will very much deprive it of its grease, as is evident from comparing the external part of the fleece with the internal. The same comparison will show how greatly such washing has impaired its fineness. The closeness of the coat of the *Spanish sheep*; compacted as it is, by its vast diffusion of grease, into almost a coat of mail, prevents the admission of the rain infinitely beyond that of any other sheep we know of; and accordingly protects the quality of the wool longer from deterioration. But even the Spanish fleece, by constant exposure to a humid climate, and to driving winds, and rains, will be penetrated, and every year become more open and hollow, and less tenacious of its native grease, and, in proportion, less fine.

and that the fleeces should be kept in the grease as long as may conveniently be.

My opinion, as to the best mode of preparing Merino wool for the market, is, that where a certain and ready sale offers, it should be left wholly in its native grease, without being washed on the sheep's back. This further advantage attends it, that the fleece is much more captivating to the eye, and the fibre appears much more silky and fine. I fear, however, that there is not, at present, that quickness and certainty of sale, which will permit the grower to produce his wool in this condition. For if they have a chance of lying a long time in the grease, they will heat and be injured. I cannot, therefore, recommend it as a general practice, but I think where wools are likely to be sold within six months of shearing, there can be no objection to keeping them in the full grease. I have, however, the satisfaction to state, that by the moderate degree of ablution, which takes place in washing the wool on the sheep's back, the grease is not expunged in a degree to injure the softness of the fibre. The same mode is practised in Saxony, and is altogther different from the complete washing in hot and cold water, which the wool receives after being shorn in Spain.

The wool may without injury be washed while growing.

The waste on British Merino wool, which has never *been washed on the sheep's back*, is rather more than one half, or about 10lbs. in 20, reckoning to its clean picked state. The same wool, *when washed on the sheep's back*, loses with the manufacturer about one third, or from 6 to 7lbs. in 20, which is about the average of the waste of Saxon wool. Whereas, the best imported Spanish wools will not waste more than half that amount: viz. from 3 to 4lbs. in 20. It is obvious, that a proportionate difference must be made in price, for the different conditions in which British Merino wools are produced; the manufacturer will be better able to estimate the probable waste of the wool that has been washed on the sheeps' back, as there is so much dirt, sand, and filth, generally with the wool in its genuine, unwashed state, that the waste must be always uncertain. I think, therefore, that wool washed on the sheep's back will be the most merchantable.

Clean wool weighs only half of native wool in the grease.

I would also remark on the most preferable mode of managing the lamb's fleece, which I should recommend cutting, in preference to remaining on the lamb, till he becomes a yearling, as practised by many. The external part of the hog's fleece, which was the original lambs-wool, suffers most materially from the inclemency of the weather and the winter. In its state of lambs-wool it is beautifully soft, but being afterwards protruded from the new coat, it is in that condition exposed to the snows, winds, and rains of the winter, by which it becomes entirely deprived of its grease, and as coarse as the wool of our common country sheep. The deterioration of this exposed part of the fleece, in one season only, fully proves what effect climate and weather have on the fibre of wool; it is therefore certainly desirable to shear the lambs, as in Spain; and although the covering may be more complete for the young sheep against the winter with the lambs coat on, yet the being rid of the incumbrance of a wet draggled fleece, in deep soils and bad weather, is of great advantage to the young and tender sheep.

Lamb's fleece should be cut early,

because it is deteriorated by exposure.

EDWARD SHEPPARD.

Uley, Gloucestershire, March 5, 1812.

General

VIII.

General Results of Beccaria's Observations upon the Electricity of the Atmosphere during serene weather; together with those of Romayne and Henley. Abstracted by a Correspondent. (R. B)

To William Nicholson, Esq.

SIR,

AFTER the systematic arrangements of clouds by M. Luke Howard, and his speculations upon their formation and disappearance, which I consider as having greatly enlarged and regulated our knowledge and means of making atmospheric researches,—and particularly from the probability that the disposition, and even the notions of clouds, may be in a great measure referable to the ordinary phenomena of electrified bodies, I have thought it would be of service to the inquiries of other observers, to send you an abstract which I made for myself, of the facts and remarks of these very diligent and faithful observers; whose works, from their extent, their dispension, and even their date, though well esteemed by philosophers, are at present less likely to be referred to. At all events, I submit to your judgment, and am, without farther preface,

Sir,
Your most obliged reader,
R. B.

Value of Beccaria's observations.

THE numerous and important observations of Father Giambatista Beccaria, on Atmospherical Electricity, render his conclusions on this subject highly estimable. His treatise annexed to the English translation of his Artificial Electricity deserves to be consulted. At present, I shall do little more than give his propositions or general results.

Apparatus, a long insulated wire, exposed to the atmosphere.

The apparatus by which those results were obtained, was settled on the pleasant hill of Garzegna, in the neighbourhood of Mondovi; from which the whole compass of the Alps, as well as the whole plain of Piedmont is easily discovered. It consisted of an iron wire one hundred and thirty two French feet long, extending from a stack of chimnies, over which it was raised by a long pole to the top of a cherry tree. Its extremities were insulated and defended by a small umbrella of tin, covered beneath with sealing wax. From this wire, another was introduced into a room through a pane of glass. It was found

Pith balls were connected with the wire.

The electricity is almost constantly positive in clear weather.

1. That the electricity, during serene weather, in its ordinary or mean state, causes two balls of pith of elder one line in diameter, to diverge six lines from a small plate of metal placed between them. The balls were suspended by very fine treads, sixteen lines long. 2. In the state of its greatest intensity the divergence of the balls is fifteen, twenty, or more degrees from the metal. 3. In its weakest state the balls move towards a conductor at a very small distance. 4. The electricity is sometimes so slow in its accumulation as to require one minute to become again sensible, after having been taken off by touching the wire; but at other times it became again sensible in the time of one second. 5. That it is always of the positive kind, excepting in some very rare instances, when the contrary happens, in consequence of the wind blowing from some other part of the sky which is not serene. The instances related by Beccaria are very curious.

He used an hygrometer of flaxen thread and another of oat beard.

Father Beccaria used an hygrometer consisting of a string of thirty-two flaxen threads twisted together to the thickness of two thirds of a line. It was twelve feet long, and the lower part passed round a pully which carried an index. The stretch-weight was two pounds. Such an hygrometer commonly served him a year, and he distinguished smaller mutations than it was capable of shewing by means of another hygrometer made of a twisted rye-stalk.

The electricity in clear weather is proportioned to the humidity.

6. During clear weather the moisture in the air is the constant conductor of the atmospheric electricity; and this electricity, is proportioned to the quantity of that moisture which surrounds the wire, except such moisture lessens the insulation both of the wire and of the atmosphere.

Beccaria observes, that he does not here pretend to point the cause or principle which produces the electricity, but only to ascertain the medium in which it is inherent, and to the quantity of which it is generally proportioned.

It is always positive at the clearing up of the weather, and more rapidly produced as the evap. is more rapid.

7. The electricity that takes place when the weather clears up is always positive. When the air takes up moisture very rapidly the intensity of the electric state of the wire, as well as its quickness in becoming again sensible when destroyed are great; but the latter diminishes as the weather becomes dryer. It sometimes happens that the electricity thus caused continues a long time in its state of intensity, and begins afresh after being interrupted. Beccaria thinks these effects are owing to electricity being brought from great distances by the wind.

Particular observations of clouds, their figures and changes, and the concomitant electricity.

8. If the sky becomes clouded over the place of observation, and only an high cloud is formed without any secondary clouds under it, and the clo[illegible]self be not part of a cloud that drops rain elsewhere, then the electricity of the wire is either positive or null. But if the clouds resemble locks of wool moving to and from each other; or if the general cloud is forming very high and is stretched downwards like descending smoke, then a frequent positive electricity commonly takes place, which is more or less strong in proportion to the quickness with which the cloud is forming, and foretels the quantity and suddenness of the rain or snow which follows. 2. When a rare, even, and extensive cloud is forming, which darkens the colour of the sky, and renders it grey, positive electricity, very intense and speedily recovering its intensity when taken off, is produced; which state diminishes and even fails as the gathering of the cloud slackens; but on the contrary, if the cloud continues to increase gradually by the accession of smaller clouds, resembling locks of wool which are continually joining and separating, the positive electricity usually continues. 3. Low and thick fogs (especially when they rise into a superior air considerably free from moisture) carry up to the wire electricity which gives frequent small sparks, and the balls diverge between 20° and 30°. If the fog seems stationary and continues to environ the wire, the electric signs soon disappear; if it continues to rise and another cloud of fog succeeds, the wire is again electrified, though less than before. Sky rockets sent through such thick low and continued fogs have often afforded our celebrated observer signs of electricity by means of a string affixed to them. He never, however, observed in any of the above circumstances, any signs of negative electricity except once by a sky rocket sent through a fog, in which he saw the

Fogs give positive signs.

Rockets made use of

star of electric light denoting negative electricity, but thinks that he might have mistaken its figure.

Romayne and Henley.

As Father Beccaria in this place mentions his two fellow labourers, Romayne and Henley, I shall here take occasion to notice their observations, and then resume my subject.

M Romayne's apparatus, an electrometer at the end of a pole.

Mr. Romayne* made his experiments between the year 1761 and 1772. He held an electrometer, consisting of two cork balls, suspended by threads six or seven inches long out of a garret window, by means of a pole five feet long; and to these, when electrified, applied excited glass, or sealing wax, by the help of another pole, and by that means determined the kind of electricity.

He found positive electricity.

He found the air at a proper distance from buildings, ships' masts, &c. to be very sensibly electrified during winter, in foggy or in frosty weather; less so in mists, and still less in calm and cloudy weather. But in summer he never observed any electricity, except during a fog in the cool of the evening, or at night. He never found any electricity during the time of an aurora borealis, unless a fog happened at the same time; excepting once, and then it was weakly positive.

He always found the electricity of the air to be of the positive kind; excepting once only, during a fog, on an uncommonly warm day in winter.

When a fog became very thick, he observed that the cork balls came nearer to each other, but opened again on its recovering its former state; and he also found, that rain during a fog produced the same effect, which ceased as soon as the rain was over.

Smell of fogs and of the electric spark.

Mr. Romayne also observed that the smell of fogs, and frequently of the common air, resembles that of an excited tube. He observes, that when the density of fogs floating near the earth increases considerably, the balls always approach; but that the reverse generally hapens when the fogs are high in the air. He once saw a struggle between breezes from N. W. and S. E. at the same time in which the one seemed sometimes to prevail and afterwards the other. The contention was preceded by a smoky haziness, like a fog, which occasioned the balls to diverge; as the haziness thickened they separated more, and the repelling power was augmented in proportion as the drops increased.

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* Phil. Trans. Vol. LXII. p. 137.

On this occasion, M. Romayne was the first who made an elegant experiment, to shew, that the diminution of surface increases the intensity of electricity in bodies. He found, by repeated trials, that a piece of flannel, silk, &c. excited and suddenly twisted, not only struck at a greater distance than before, but sometimes emitted parcels of fire into the air. And from this he infers, that the electricty of vapour, when not in contact with the earth, ought to increase by condensation. This is still farther confirmed by the experiments of Volta and of Bennet, on the electricity of vapour*.

Sudden changes of electricity, which might be explained by correspondent observations.

At other times, M. Romayne made use of a tapering tube of tin, twenty feet long, and ending in a point, insulated, and projecting upwards out of a window. He took notice of that uncertainty and frequent change in the electricity of clouds, which was before remarked by Dr. Franklin and others; and, after several ingenious observations, he expresses his wish, that two or more persons, at a sufficient distance, would correspond by signals, indicating positive electricity by a red flag, and negative by a blue; as it is highly probable that much more satisfactory knowledge would be thus obtained, respecting the electricity of the clouds, thunder, &c. than any single observer could acquire.

The observations of Mr. Henley † tend to corroborate those of Mr. Romayne, but do not lead to any further conclusions.

General facts.

I now proceed in the enumeration of general facts, or the propositions of Beccaria.

Single clouds diminish the intensity in the wire. Separate masses increase it.

9. In clear weather, when a low cloud, considerably distant from any other, happens to pass slowly over the wire, the positive electricity is usually much diminished, but is not rendered negative; and, when the cloud is gone, it returns to its former state. But, if numbers of whitish clouds, resembling locks of wool, continually uniting and separating, remain over the wire, so as to form a considerable extent, the positive electricity

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[*] And more fully by the condenser and well-known experiments, made with Bennet's gold-leaf electrometer.

[†] Ph. Trans. vol. 62. p. 145, and vol. 64. p. 422.

city commonly increases. The electricity never becomes negative in either of the above cases.

Father Beccaria, in his experiments on the electrified air of a room, found that the electricity is proportional to, and therefore most probably resides in the vapours floating therein. The same conclusion may, therefore, as he observes, be naturally applied to the atmospherical electricity, which is not sufficient in general to produce electric figures, in electrometors which are not insulated. The two last propositions, 8 and 9, relate to such phenomena as take place when the weather either becomes overcast or clears up. The following relates to the effects of vapour or moisture, as shewn by the hygrometer.

The artificial electricity in a room is proportioned to its moisture.

10. In the morning, if the hygrometer indicates a great degree of dryness, very little difference from that of the preceding day, then even before sun-rise an electricity takes place, causing junction, adhesion, or divergence, of the ball; and its intensity is greater the drier the air, and the less that dryness differs from that of the preceding day. But if no such great dryness obtains, no perceptible electricity takes place, till sun-rise, or a short time after.

In very dry mornings electricity is shewn; but if the air be not very dry, the electricity appears after sun rise.

11. The electricity of the air gradually increases as the sun rises higher. The gradual increase begins sooner, according as the hygrometer continues after sun-rise to indicate a higher degree of dryness, and as such dryness more speedily increases. This increase, both of intensity and speedy recovery, when taken off, last in serene days, when the wind is not violent, till the sun draws near its setting, provided the hygrometer keeps near the highest degree it has reached. But when the sun is near setting, and in proportion as the hygrometer retreats, the intensity of the daily electricity is diminished, at the same time that the quickness with which it is revived in the apparatus, when taken off, becomes greater.

The electricity increases as the sun rises higher, &c.

12. Though the hygrometer may indicate equal degrees of dryness in the middle of the day, on different days, yet the time in which the apparatus recovers its electricity on those days is less, the greater the increase of heat; and when the heat is greater, the electricity arises later in the morning, and fails sooner in the evening

Difference when the moisture and heat are differnt.

13. The friction of winds against the surface of the earth is not the cause of atmospheric electricity. Impetuous winds dimi-

Winds do not produce electricity by friction.

nish the intensity of the electricity of clear weather. And if they be damp they diminish its intensity, by rendering the insulation both of the atmosphere and of the apparatus more imperfect.

Other facts and observations on the friction of winds.

Father Beccaria made many experiments to discover whether the friction of air against conducting bodies produced electricity. He used the bellows, and also turned fans of gilt pasteboard very swiftly round on an insulated axis, but obtained no electricity either in damp or dry weather. He had before observed, that air produces electric signs, when it strikes their glass*. He found also that the umbrella, with an insulating handle, which the French call paratonneres, never exhibited the least electricity when held obliquely to the wind. To these I may add, that the very sensible electrometer, of Bennet, does not become electrified by blowing pure air upon it†. The proposition of Beccaria does not, however, rest upon electrical experiment, but is likewise supported by a variety of actual observations on the state of the atmosphere. And though these cannot be transcribed, on account of their length, yet I am unwilling to pass over in silence his very cogent remark, that if the electricity in any degree arose from the friction of winds against the ground, it would be found the greatest near the surface of the earth, but the contrary is the fact.

Night electricity in cold dry air,

XIV. In cold weather, if the sky be clear, the wind not violent, and the air considerably dry, an electricity of considerable intensity arises after sunset, as soon as the dew begins to fall. The quickness with which the apparatus recovers its electricity after being touched, is greater than during the diurnal electricity, and it disappears very slowly.

and also in temperate or warm weather.

XV. In temperate or warm weather, and in the same circumstances of wind and moisture, an electricity perfectly similar to the above takes place as soon as the sun has set; but its intensity is not so constant, it begins with more quickness, rises to a state of more speedily recovering its intensity after being touched, and ends sooner.

Moisture affects the insulation.

XVI. When the air in the above circumstances is less dry, the electricity is less intense, by reason of the insulation being rendered more imperfect, but its quickness in recovering its intensity after contact, is greater, as the quantity of dew is greater.

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* See p. 363, vol. I. on Artificial Electricity. § 776.

† Ph. Trans. vol. LXXVII. p. 30.

Dew acts nearly like rain.

XVII. The electricity of dew seems to be, in proportion to its quantity, in the same manner as the electricity of rain depends on its quantity; and the peculiar manner or circumstances which attend the falling of the dew, influences the electricity in the same way, as does the peculiar manner in which rain takes place.

Succession of similar phenomena.

XVIII. As rain, showers, aurora borealis, zodiacal light, have a tendency to begin afresh for several successive days, with the same characteristic accidents, so the electricity of dew seems to have, as it were, an inclination to appear for several evenings successively, with like characters.

An experiment of artificial dew in a room artificially electrified.

After these propositions relating to the dew, father Beccaria adds the following: let the air, in a closed room, be electrified, that is to say, the moisture and other vapours diffused in it; let a bottle filled with water, colder than the air of the room, and insulated on a stove of glass, be raised pretty high in the room, and the insulation be carefully preserved. Then the electric signs that will arise in two threads suspended to the bottle, will exactly represent the electricity of dew, for they will exhibit the different manners after which this electricity takes place, according as the electrified vapours in the room are more or less rare; as the difference between the heat of the bottle, and of the air in the room, is more or less; and as the insulation of the bottle is more or less accurate.

This excellent and most industrious philosopher, after reciting various facts respecting the electricity of dew, concludes with the following summary observations:

The diurnal electricity resembles that of a fog, and the night electricity that of a shower.

The diurnal electricity resembles the electricity of a very rare fog, which rises, becomes dilated, and by that means, continually renders the insulation more perfect. The nocturnal electricity resembles that of a very rare and subtle rain, which descends, becomes condensed, and continually renders the insulation less perfect, whenever the diurnal electricity is more constant. But the nocturnal electricity frequently fails, and only attains its greatest intensity when the increase of that moisture, which is the conductor of it, happens to take place without injuring the insulation.

Notice

IX.

Notice of an Adventurer to the Interior of Africa.

Interior of Africa.

IT was some time since mentioned, that a German, of the name of Roentgen, had been making preparations to prosecute the same objects of discovery that excited the ardour of the celebrated, though unfortunate, Park; and, penetrating into the central regions of Africa, to reach, if possible, the city of Tombuctoo, which has never yet been explored by any European traveller. The following article on this subject has appeared in a German journal of the 8th of October, quoted in the General Chronicle.

"There has been lately published, at Nenwied, an interesting letter from the traveller Roentgen to his brother. It reached him through Professor Hagen, who received it from Mr. Nunemann, of London. Roentgen, it appears, after visiting Paris, Vienna, and London, had repaired to Mogadore, where he resided a considerable time; and the letter in question, dated the 21st of July, 1811, was written on the bank of the river Teulifft, at the moment of his departure for the interior of Africa." The following is some of the most interesting information it contains:—

'During my residence at Mogadore, I was engaged day and night in studying the Arabic; and I have succeeded in making myself to be understood by the natives of the country. I will avail myself of that knowledge of the country, and of the manners of the people, which I have acquired, in order to travel to Tombuctoo. I would not act with so much boldness, were I not convinced, that providence has destined me to make the discovery of the interior of Africa. My good stars have furnished me with a companion in my travels, than whom I could not have wished for a better. He is a German, who, when only twelve years old, quitted his paternal roof, having an irresistible inclination for roaming; he has never since lived six months on the same spot, and is now thirty-eight years of age. He knows all the European languages, the Sclavonic excepted. Fourteen years ago, when destitute of money or protection, he was impressed by the English for a sailor, in an island of the Mediterranean, Interior of Africa.

Mediterranean, where he happened to be; he was inhumanly treated by them, and reduced almost to despair. His ship anchored before Tetuan for the purpose of watering; and there, having struck an English officer who had used him ill, in order to avoid punishment, he escaped, and became a Mussulman at Tetuan. Since then, he has traversed the Barbary states in all directions, and has lately returned from a pilgrimage to Mecca. He has lived at Jamba, in Africa, as a coffee-house keeper, and at Janol, as a physician. At Constantinople, he has superintened the gardens of a Pacha. I got acquainted with him at a merchant's in Mogadore, who had hired him as a gardener. I have taken him into my service, and I treat him rather as a friend than as a domestic; the benefits which I shall derive from his experience are immense.

'About a month ago, I travelled with a caravan of merchants to Morocco, where I procured valuable information respecting the communications with the interior of Africa.

'It is impossible to convey an idea of the violent hatred which animates the Moors against Christians. Even at Mogadore, I could hardly go abroad without being overwhelmed with insults. I was obliged, in order to view the city of Morocco, to get an escort of four soldiers, who, by orders of the government, were to keep back the populace. Even then I was often assailed by stones, one of which hit me so severe a blow on the forehead, that for some time I thought myself dangerously wounded. This hatred of the Moors arises in a great degree from our dress.

'I saw, at Morocco, preparations for the setting out of a caravan, which was to reach Tombuctoo by Tafilet and Tunt. I immediately formed a resolution to join this caravan, and I returned to Mogadore. My companion was delighted with the plan, which I did not communicate to any one else, but to one Christian. I caused it to be reported at Mogadore, that, disgusted with the bad treatment I had received at Morocco, I meant to repair to Tangier, and from thence embark for Gibraltar. This pretended project furnished us with a pretext for purchasing a mule, and every other necessary for my journey. I secretly procured some Moorish garments. Having finished my preparations, I invited some Christians at Mogadore to a party of pleasure on a mountain, about six English miles off, whither

Interior of Africa.

they were often in the habit of going. I have there spent one day with them, and declared that I meant to proceed for Tangier. They will accompany me to a certain distance, and give out at Mogadore that I am on my way to Tangier. As soon as I am left alone with my fellow-traveller, I mean to clothe myself in my Moorish garb, and to enter the great road which leads from Tafilet to Morocco. From thence I shall reach Deminit, a town situated at the foot of Mount Atlas, where I shall be safe from any searches which the governor of Mogadore might make, should he learn that I have not gone to Tangier. At Deminit, I shall join a caravan, which will pass there about that time, and with it I shall cross Mount Atlas, covered with snow, and next enter the burning plains of Tafilet. I shall remain at Tafilet with a German renegado. There are in that city a number of Germans. There are some Germans in Morocco, and to one of them I am indebted for some valuable information. I expect to find a German in Tombuctoo, and there I mean to remain six months, making it the centre of my observations on the interior of Africa. I shall pass for a physician: I have laid in a supply of medicines, of which I know the application. It is my wish to penetrate towards the south, and to be able to reach Wescmb, or the Cape. Should I find this too difficult, I mean to return to Europe to publish the journal of my travels; and shall again return to Africa, where I am destined to make some discoveries.'

X.

Description of a remontoire Escapement for Pendulum Clocks, invented by Mr. George Prior, *Jun**.

New escapement.

THE swing wheel, A, figs. 1 and 3, Plate III, has thirty teeth cut in its periphery, and is constantly urged forwards by the maintaining power, which, in the model represented in the engraving, is supplied by a small weight, X, figs. 2 and 3; CD are two spring detents, catching the teeth of the wheel alternately;

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* Soc. Arts, XXIX. anno. 1811. The society bestowed a premium of 20 guineas for this invention.

New escapement.

alternately; these are, at the proper intervals, unlocked by the parts marked 2 and 3, fig. 1, upon the pendulum rod H, intercepting small pins, *a b*, fig. 2, projecting from the detents, as it vibrates towards the one or the other; E is the renovating or remontoire spring, fixed to the same stud F as the detents. It is wound up by the highest tooth of the wheel, as seen in fig. 1, (its position when unwound being shown by the dotted line) This being the case, suppose a tooth of the wheel is caught by the detent D, which prevents the wheel from moving any further, and keeps the renovating spring from escaping off the point of the tooth: in this position, the pendulum is quite detached from the wheel; now, if the pendulum be caused to vibrate towards G, the part of it marked 2, comes against the pin *b*, fig 2, projecting from the renovating spring E, and pushes this spring from the point of the wheel's tooth; on vibrating a little farther it removes the detent D, which detained the wheel by the part 3 striking the pin (*a*, *fig*. 2) which projects from the detent; the maintaining power of the clock causes the wheel (thus unlocked) to advance, until detained by a tooth resting upon the end of the detent C, on the opposite side; by this means, the renovating spring will be clear of the tooth of the wheel as it returns with the pendulum, and gives it an impulse, by its pin *b*, pressing against the part 2 of the pendulum, until the spring comes to the position shown by the dotted line; in which position it is unwound, and rests against a pin fixed in the cross-bar of the plate; the pendulum continues vibrating towards I, nearly to the extent of its vibration, when the part 1 meets the pin in the detent C, and removes it from the wheel and unlocks it; the maintaining power now carries it forward, pushing the renovating spring E before it, until another tooth is caught by the detent D, which detains the wheel in the position first described, the renovating spring being wound up, ready to give another impulse to the pendulum.

N. B. The pin *b*, fig. 2, is not fixed to the renovating spring itself, but is part of a piece of brass, which is screwed fast to the renovating spring, and is made very slender near the screw which fastens it; this permits the end of the renovating spring to give way, if, by the weight being taken off the clock, or

any

New escapement.

any other accident, the escape-wheel should be moved backwards, so as to catch on the detents improperly.

The following observations are necessary to be attended to in this escapement.

1st. That the renovating and detent springs must spring from one centre, and as similarly as possible.

2d. That the force applied to the train must be so much more than what will wind up the renovating spring, as will overcome the influence of oil and friction on the pivots of the machine.

3d. That the renovating spring, when unwound, must rest against the point of the tooth of the wheel; which will be an advantage, as it thereby takes as much force off the tooth of the wheel resting against the detent spring, as is equal to the pressure of the renovating spring C, against the face of the tooth of the wheel.

4th. The detent springs must be made as slender and light as possible; though whatever force they take from the pendulum, by their elasticity in removing them, to unlock the wheel, so much force they return to the pendulum in following it, to where it removed them from; therefore action and re-action will be equal in contrary directions.

5th. That it is unnecessary for the pendulum to remove the detent or renovating springs, much farther than is necessary to free the teeth of the wheel, as it will always vibrate up to the same arc; in table clocks it ought to remove them further, so that it can go when not placed exactly level, or what is generally termed, out of the beat.

XI.

Description of a simple, cheap, and easy Method of preventing the Annoyance of steam from Boilers in Manufactories and other Places. By MR. GEORGE WEBSTER, *of Leeds*[*].

Easy means of conveying steam and vapor up a chimney.

THE introduction of steam into workshops and manufactories, is injurious to the articles, to the buildings, and to the workmen; and, when the matter evaporated from boilers is of an offensive nature, it must be still more desirable to dissipate, or carry it off, in the most efficacious and simple manner. Mr. Webster, after various trials, has accomplished this by an ascending trunk or pipe, which communicates with the chimney, and is explained in the following description, by reference to fig. 4. Plate III.

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[*] For which the Soc. of Arts gave their silver medal in 1811.

A A, the brick work surrounding the pan.

B, the steam chimney, made of wood, about two feet broad and six inches deep. A small opening at the back part of the pan admits the steam into this chimney; it may from thence be carried up to the top of the building, or turned into any smoke chimney near at hand.

In order to keep the water in the pan as hot as possible during the night, there are two dampers in the steam chimney at D, and if both these dampers are shut, and the whole top of the pan covered closely over at *c*, the boiling water, even when the fire is withdrawn, will keep hot for the workmen till the next morning.

C C, are loose boards, fitting close to each other, and covering completely the better half of the circle of the top of the pan; and upon this circumstance depends the whole secret of getting quit of the steam. If you remove these boards or partial coverings, the steam chimney loses all its use. The letter *b* shews the part of the top of the pan which should be left open to admit to the workmen a ready communication with the hot water; and through this open part a current of cold air is constantly seen to press and force the steam rapidly up the steam chimney.

It is proper to add, that there must always be an empty space of two or three inches between the surface of the hot water and the under part of the cover *cc*, so as to permit the steam to pass to the bottom of the steam chimney. To effect this purpose, and at the same time to allow the copper to be full of hot water, a rim or curb of wood F, about three inches thick, should be fixed on the top of the copper, and upon this the covering boards *cc* placed. This allows sufficient room for the steam to press forward to the steam chimney at all times.

The cover and wood steam chimney are removeable, and may serve for another copper, if both be not wanted at the same time.

METE-

XII.

METEOROLOGICAL JOURNAL.

1812.	Wind	Barometer. Max.	Min.	Med.	Thermometer. Max	Min	Med.	Evap.	Rain	
11th Mo										
Nov. 25	S W	29·89	29·80	29·845	48	35	41·5			☽
26	E	30·07	29·77	29·920	47	43	45·0		0·26	
27	N	30·21	30·10	30·155	49	42	45·5			
28	N E	30·10	29·92	30·010	47	38	42·5			
29	S E	29·89	29·85	29·870	49	41	45·0			
30	S	29·95	29·88	29·915	50	47	48·5		0·15	
12th Mo.										
Dec. 1	S	29·96	29·72	29·840	52	44	48·0		5	
2	N W	30·08	29·96	30·020	49	42	45·5		—	
3	E	30·11	30·09	30·100	49	45	47·0		—	●
4	E	30·08	30·05	30·065	48	38	43·0		0·11	
5	E	30·22	30·08	30·185	44	33	38·5			
6	N E	30·51	30·29	30·400	42	26	34·0			
7	N E	30·51	30·41	30·460	35	22	29·0			
8	N E	30·41	29·94	30·175	34	18	26·0			
9	W	29·96	29·94	29·950	35	24	29·5			
10	N W	29·89	29·78	29·835	34	29	31·5		—	
11	E	30·00	29·97	29·985	36	27	31·5			☾
12	N E	29·97	29·79	29·880	32	24	28·0			
13	N E	29·79	29·71	29·750	34	24	29·0			
14	N E	29·71	29·66	29·685	35	28	31·5			
15	E	29·66	29·20	29·430	34	28	31·0			
16	E	29·20	28·98	29·090	34	28	31·0		—	
17	E	29·22	28·98	29·100	35	32	33·5		0.27	
18	E	29·51	29·22	29·365	38	33	35·5		0·18	○
19	E	29·57	29·47	29·520	38	35	36·5			
20	E	29·76	29·57	29·665	36	31	33·5			
21	N W	29·82	29·76	29·790	38	32	35·0		—	
22	Var.	30·02	29·82	29·920	42	33	37·5		—	
23	N	30·30	30·02	30·160	36	31	33·5		—	
24	N	30·46	30·30	30·380	35	32	33·5		3	
		30·51	28·98	29·882	52	18	36·68		0·95	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eleventh Month, 28. The sky, about sunset, was overspread with *Cirrus* and *Cirrostratus* clouds, beautifully tinged with flame colour, red and violet. 30. a. m. The sky again much coloured.

Twelfth Month. 5. The weather, which has been hitherto mostly cloudy, with redness at sunrise and sunset, begins now to be more serene. 6. Hoar frost. 7. A little appearance of hail balls on the ground. 8, 9. Clear, hoar frost. 11. Snow this morning, and again after sunset. 13. An orange-coloured band on the horizon this evening; this phenomenon arises from reflection by the descending dew. 15. A gale from N. E. unaccompanied by snow, came in early this morning. 16. a. m. The wind has subsided to a breeze, and there now falls (at the temp. of 27·5) snow, very regularly crystallized in stars. 17. a. m. It snowed more freely in the night, and there is now a cold thaw, with light misty showers. 18. A little sleet, followed by snow. Ice has been formed in the night, by virtue of the low temperature which the ground still possesses. A wet evening. 21. A little rain, a. m. 22. A dripping mist. 24. Cloudy; a little rain; some hail balls in the night.

RESULTS.

Prevailing winds easterly.

Barometer: greatest observed elevation, 30·51 in.; least 28·98 in.; Mean of the period 29·882 inches.

Thermometer: greatest elevation 52°; least 13°. Mean of the period, 36·68°.

Rain and snow 0·95 inches. The Evaporation during this period has not been ascertained.

PLAISTOW,
First Month, 7, 1813.

L. HOWARD.

XIII.

An Explanatory Statement of the Notions or Principles which the Systematic Arrangement is founded, which was adpoted as the Basis of an Essay on Chemical Nomenclature.*

By Professor J. BERZELIUS.

The affinity of chemistry explained.

WE may consider the affinity of chemistry as a tendency by which bodies are incessantly urged. By this affinity they are disposed to combine, in such proportions and in such numbers of each body, that they afterwards cease to manifest any farther affinity of combination, which property may from that period be considered as in a state of repose or inactivity. Such a combination or compound, which no longer shews any affinity towards the greater part of other bodies, may be itself called an *indifferent body*. If, for example sulphur, caritium, and oxigen, come into contact, they tend to combine in such a manner, as to produce sulphate of barytes; and in this compound the affinities of the ingredients appears to be in a state of repose; that is to say, they constitute an indifferent compound.

Absolute or complete union of principles,

The entire tendency or activity of the affinity is, therefore, exected to arrive, by an effect, which occupies a longer or shorter time, according to circumstances, at this state of repose or indifference. If the elementary bodies were collected at the same place, and all possessed an equally strong chemical affinity, an active chemical phenomenon would ensue, which would terminate in eternal-repose. No force would tend to change this state of repose, and such different combinations as might be thus formed, being attracted by each other, by the means of gravitation and cohesion, would constitute a mass or aggregate of indifferent bodies.

does not appear in our observations.

But this is not the construction which takes place in nature, among the surrounding bodies of that small part of the universe which is submitted to our observation. A series of mutations take place in unorganized matters, by which organized nature is supported, and we have plausible reasons to conjecture, that a similar disposition prevails in the other parts of the immensity of the universe.

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* The Essay was published in the Journal de Physique, of Dr. Delametherie, Oct. 1811, or Tome lxxxiii. 253. It abounds with new and interesting observations; and it is with regret that I am prevented by its length from inserting it in our work.—The present memoir, which is of considerable extent, is taken from the Memoirs of the Academy of Stockholm for 1812.—W. N.

because prevented by caloric, light, and electricity.

The circumstances which incessantly tend to destroy or to prevent the repose of combined elements, are light, caloric, and electricity, assisted by the circumstance that the chemical affinity of the different elementary bodies is not equally strong.

These have a mutual relation.

Caloric, light, and electricity, have a mutual relation with each other; easy to be perceived, but very difficult to be comprehended. Very often the presence of one of these produces the other, without one being capable of determining whence it comes. When a large and powerful electrical pile is discharged by means of two points of platina, a sun is produced at the point of discharge; indeed upon a scale of infinite minuteness as to magnitude, but which, by the intensity of its light and heat, surpasses every other phenomenon of fire produced upon one globe; which fuses the metal, and loses nothing by the comparison, even when produced in the midst of a flame, supported by oxigen gas. The production of light and heat at the point of the electric discharge; that is to say, at the point where the two separated electricities it cease to manifest themselves as electricity, cannot be mistaken; and proves, that there is a relation between these substances, which we may, perhaps, hereafter be better able to comprehend than at present.

The union of electricities produces light and heat.

Tendencies to equilibrium of caloric and electricity,

Caloric and the electricities exhibit, in our experiments, a kind of tendency to acquire an equilibrium; that is to say, to arrive at the same state of repose, which appears to be the ultimate end of the chemical affinity of ponderable matter. But this equilibrium of caloric and the electricities is incessantly broken by the rays of the sun, by which the surfaces of the planetary bodies is alternately enlightened at determinate intervals.

which is disturbed by the solar light.

There is therefore a process carried on in the sun, by which the repose of the united elements is incessantly intercepted or prevented, and which preserves them in a certain state of activity. It is impossible for us to determine the nature of this process; because the truth of ou rconjectures will never, in all probability, be proved in a satisfactory manner; but, notwithstanding the difficulty, it will always be a subject of interest to ascertain which of our conjectures may be the least improbable.

Common fire is produced; 1. By combination of bodies; 2. By union of electricities.

We know that the phenomenon of fire is produced on one globe on two principal or leading occasions. 1. When two bodies combine; for example, in oxidation, sulphuration, the combination of acids with bases, &c.; and 2. When the separated electricities mutually penetrate each other, and cease to appear as electricities.

Friction and concession.

(There are, nevertheless, two other manners by which fire may be produced; namely, friction and compression. As to friction, there is reason to believe that it will be found to class itself along with the electric discharge; and compression, on the other hand, does nothing more than drive the caloric out of a body which it contains already produced. But in the present part of our discussion, we attend only to the cases in which caloric appears to be produced, that is to say, in which we cannot conceive whence it comes).

The sun is in a state of combustion,

It is incompatible with every scientific notion we possess, that the phenomenon of an interior fire should be produced in the sun, by a chemical combination, or by a condensation of ponderable substances. Such an opinion has been rejected by our ancestors, though their notions of combustion were less precise than ours; and it appears to be contradicted by the circumstance, that the magnitude of the mass of the sun remains constantly without alteration, at least, as far as our observations can determine. It remains therefore as the least improbable of our conjecture, that a process is affected in the sun, analogous to that which obtains between the points by which an electric pile is discharged; and we must imagine that this process, when once commenced, must, from the nature of the actual arrangement of things, continue for ever; and that, consequently, the activity of created matters is maintained, as it were, by a gyration in a circle, or by always returning again to their first situation or state, as in astronomy we know to be the case with their motions in space. It is beyond the limits of human reason to determine how these processes at first began, and it would no doubt be unworthy of an enlightened and discerning mind to presume seriously to form any conjecture upon the subject.

and may therefore be supported in a state of continued electric discharge.

The electrici-

Our experiments with the electric pile, have proved how much the Electricities are concerned in the operations of chemical affinity; and that sometimes they are suppressed, and at other times made to act in an opposite sense. It was even observed, before the discovery of the Electric pile, that the Equilibrium of Electricity is sometimes disturbed by chemical operations, and the knowledge acquired from the labours of the last ten or twelve years, has shown us, that there is not a single action of affinity, in which the electricities do not co-operate.

ties are concerned in the operations of affinity,

We do not know how this co-operation is made, and, for the moment, we must be satisfied with conjectures upon it. What we with certainty know is, that two bodies which have affinity for each other, and which have been brought into mutual contact, are found upon separation to be in opposite states of electricity. That which has the greatest affinity for oxigen usually becomes positively electrified, and the other negatively. Bodies which have little affinity between them, or, which have nearly an equal affinity for oxigen, do not sensibly derange the electric equilibrium by their mutual contact. This is not only the case with combustible bodies, but it also takes place with the oxides; as for example, the oxalic acid, dry and deprived of its water of crystallization, brought into contact with quick lime, becomes, according to the experiments of Davy, negatively electric, while the lime becomes positive. And since the electric state of these bodies is more marked, the higher the temperature, that is to say, as the chemical affinity becomes more active; and lastly, as at the moment of their union there is a production of heat, which may vary from a very slight elevation of temperature to that of the most intense fire, we think we may conclude, that at the moment of the chemical combination, there is a discharge of the opposite electric state of the bodies, which here, as in the pile, produces the phenomenon of fire, at the instant when the electricities disappear.

in a manner not known;

but bodies having affinity, shew the electric states on separation,

that bodies becoming positive which has the strongest affinity for oxigen.

Elevated temperature increases the affinities and the electricity, and at the instant of union, heat ensues probably from an electric discharge.

A derangement of the equilibrium of electricity appears therefore to precede, and as it were predispose, the action of the chemical affinity; though this phenomenon from physical reasons cannot be always discovered by our instruments; as may happen, for instance, when one of the bodies is in the liquid state. Davy found, in conformity with this, that sulphur, heated

A change in the electric equilibrium precedes the action of ch. affinity.

upon copper, gave signs of very strong electricity, constantly increasing with the temperature till the sulphur melted, at which instant the signs disappeared,

After ponderable bodies have combined, they can be separated only by the electricities acting peculiarly on each body.

After the union of ponderable bodies, in which the electricity is seen to fly off in the form of light and heat, the ponderable bodies are reduced to a state of chemical repose. The elements of the combination can no more be separated, nor be restored, to their original form and characters, without the influence of a mass of electricity, in a state of charge or of separation, as in the operation of the pile. But in this case the electricities, tending to regain their equilibrium, decompose the combination, by operating each upon its relative constituent part to which it restores its original form and characters.

(*To be Continued.*)

XIV.

Facts and Remarks, upon the Interruption which the situation of the maintaining weight produces in the rate of a clock, when near the pendulum. By H. K.

To Mr. Nicholson.

SIR,

Rate of clocks affected by the position of the weight,

IN your Journal for October last, I observed a paper by Mr. Thomas Reid, on the effect produced on the going of clocks, by the attraction between the weights and the pendulum.

was observed and remedied by professor Hornsby.

The effect alluded to, viz. that of the arc of vibration becoming less when the weight is near the ball of the pendulum was remarked some years since, by the late Dr. Hornsby. This gentleman having done me the honour to accompany me in a visit to the observatory at Oxford, pointed out an astronomical clock there, the weight of which he had contrived to pass behind the clock case. He informed me, that he had remarked an irregularity in the going of the clock, when the weight approached the ball of the pendulum, and attributed it to the increased resistance of the air, from its free motion being impeded by the weight of the clock.

Reason why this effect is not to be ascribed to attraction.

Indeed, it does not seem that attraction could produce the effect alluded to; for, though the ball of the pendulum might be retarded in its ascent, its motion would be proportionably accelerated on its return.

I have been induced to trouble you with this, merely from respect to Dr. Hornsby's memory, and not with the slightest intention of depreciating the talents of Mr. Reid.

I am Sir,

Your obedient humble Servant,

H. K.

Ipswich, Dec. 6, 1812.

REMARK.

Additional obs. on Mr. Reid's communication.

From the nature and tenor of Mr. Reid's communication I concluded, that his single weight descended either in front or behind the ball, and not on one side of it; and in this arrangement its attraction would add to that of gravity, whether perceptibly or not. I likewise requested his brother, who brought the paper, to suggest that it might be desirable to make trial of a temporary piece or mass, to be put on or taken off at pleasure, in the place where the weight had been inferred to produce the greatest acceleration; and to keep the weight out of the limit of disturbance; this would remove all suspicion of irregularity in the train: And I would, from the ingenious observations of my Correspondent, suggest farther, that the temporary piece should be a thin shell of brass, with a solid core of lead; which, when taken out, would greatly diminish the attraction, but not the impediment from increased resistance of the surrounding air.

SCIENTIFIC NEWS.

Mountains of Lapland, &c.

Valenberg's Journey for examining the mountains of Lapland.

AN *Account of a Journey*, undertaken in 1807, by M. Valenberg, has been published lately, at Stockholm, under the auspices of the Academy of Sciences of Sweden, *for the purpose of determining the height of the mountains of Lapland, and observing their temperature.* The mountains visited by M. Valenberg, make part of the great chain which runs through Sweden and Norway, and stretches in some of its branches even to Finland and Russia. They are situated between 67 and 68 degrees north latitude, and belong to the polar regions. On several points their bases are washed by the sea, and from their summits the immense plain of the Northern Ocean is discoverable. These mountains had been only hitherto viewed in all their majestic grandeur by the Lapland nomade, following his flocks of deer and his game. A few travellers had contemplated them at a distance; and M. de Bruck, a learned German, during his travels in Norway, approached within a short space of them; but no person had ever yet penetrated into this asylum of nature, and attempted to struggle with the difficulties of ascending these summits, eternally covered with snow and ice.

The undertaking was difficult in many respects. The ascents were mostly excessively steep, and in climbing them the traveller was by turns suspended over deep fissures, lakes, torrents, bottomless marshes, and gulfs. He had no intelligent guide, there was no habitation on his route, and no assistance to be expected. He frequently was obliged to make circuits of many leagues to reach a summit; and he crossed not only snow and ice full of crevices, but also marshes, where he run a continual risk of being buried in the mud and stagnant water. He passed the nights on naked rocks, without a tent or the smallest shelter; and he was frequently reduced to quench his devouring thirst by swallowing snow, which occasioned him inflammations and painful suppurations in the mouth.

M. de Valenberg's measurements give the Lapland mountains an elevation of from 5 to 6,000 feet above the level of the sea. Although this elevation is less than that of the mountains of Switzerland and the Pyrenees, all the phenomena of the Alpine regions, and particularly glaciers, are observable. At such a proximity to the polar circle, the region of eternal snow commences at nearly 4,000 feet above the ocean, while in the Alps it begins at from 7 to 8,000, and in the Pyrenees at 8,000 feet.

On the 14th July, M. de Valenberg ascended the most considerable glacier, called *Sulitelma*, a Lapland word, which signifies Solemn Mountain, because formerly the Laplanders adored on one of its summits their principal idol. This mountain, which is the *Mount Blanc* of the north, is composed of a succession of summits, of which the base has an extent of several leagues. Its greatest elevation is 5,700 feet above the sea. To reach this elevation, our traveller was obliged to make his way over enormous crevices, where recently before some hunters had been engulphed with their deer and their dogs. Seas of ice have descended into the vallies 700 feet below the line of snow. There is a border of earth surrounds the ice, consisting of slime and stones. The ice of Sulitelma is very clear, and almost transparent; it is as hard as stone, but not so heavy as the ice of the sea. The traveller gives several details respecting its internal composition, the figures by which it is characterized, and the crevices formed on it. The snow is sometimes 100 feet in depth, and so hard that the footsteps leave no mark on it. That which is detached from the summits, or crevices, roll to immense distances. Fortunately, these avalanches in their descent act only on inanimate nature: whatever direction they take they seldom encounter living beings, or the abodes of men. All is desert in these regions for vast extents, where industry has gained no conquest over the solitary domain of the primitive creation.

The traveller terminates his account by general considerations on the temperature, and by tables of meteorological observations. He determines with precision the different regions of the mountains, and characterizes them by the productions which he found there. In proportion as the line of snow is approached, the productive force of nature diminishes, and men, brute animals, and plants, yield to the rigour of the cold. At 2,600 feet below the line, the pines disappear, as well as the cattle and habitations. At 2,000 feet the only tree is the birch; and its degraded form and indigent verdure attest the inclemency of the climate; at the same time the greatest number of wild animals disappear, and the lakes contain no fish. At 800 feet below the same line of snow, the Laplander's progress is stopped for want of moss for his rein-deer. Above the line every thing presents the picture of agony and death. The most robust lichens are only to be found at 1,000 and 2,000 feet, in the crevices of perpendicular rocks; and the bird named *emboriza nivalis*, or snow-bird, is the only living creature to be seen. The heat does not rise to one degree of Réaumur, in the region, which is 5,000 feet above the sea.

Mr. Fiddler, a captain in the Hudson's-bay service, has communicated to Mr. Arrowsmith, the draught of the district of country which lies between the rocky mountains and the great ocean, and between the latitude 52 and 46. It contains all the head waters of the Columbian River; of a lake, called, by Mr. Fiddler, Lean's Lake; a river running into it, called Arrowsmith's River; and a river of magnitude, called Wedderburn's River. The whole tract is inhabited by tribes of flat-head Indians, otherwise called Têtes de Boules, and one large extent is filled with wild horses. Mr. Arrowsmith purposes to introduce these discoveries into his General Map of North-American Discoveries.

Mr. Arrowsmith has completed a new Map of Germany, in six sheets of double elephant, being the largest map of that empire ever drawn and published in England. Like all the maps of this eminent geographer, this new one is derived either from original or unquestionable and superior sources.

The same geographer has for some years been engaged on a Map of England and Wales, in 18 sheets, which, when put together, will be 10 feet by 12. Of this extraordinary map it deserves to be noticed, that it will contain at least 1,000,000 names, which is the more remarkable because the places enumerated in the Population Return are only 15,741; and Capper's Topographical Dictionary does not contain above 20,000 places for the three kingdoms, although double the number contained in Luckombe's Gazetteer.

It is with regret that I find myself under the necessity of taking notice of some passages in the preface to Dr. Thompson's Annals of Philosophy, in which he animadverts upon the English Philosophical Journals.

1. Of my Journal he says, " that for several years it was " excellent," and adds, " that for some years past, *if report " says true*, it has not been the property of the original editor, " but of a bookseller; and, in reality, edited, not by Mr. " Nicholson, but by some unknown person employed by the " bookseller.

2. Of the *Philosophical Magazine*, which he calls a rival publication, he says " it is edited by Mr. Tullock, a printer " from Glasgow, and publisher of the evening newspaper, " called the Star," and that " it, perhaps, never contained so " much original matter as my Journal.

3. Of the *Repertory of Arts*, he says it consists chiefly of specifications of patent inventions, with a few additional papers copied from the Transactions, or other Journals; but he overlooks the remarks and discussions from the inventors and others, which are inserted in that work.

4. And of the *Retrospect* of Discoveries; or Abridgment of periodical and other Publications, he says, that it is, as the title implies, *merely* an abridgment of the other three Journals, of the British Transactions, and of *one or two* French periodical works. But, in so doing, he denies the existence of those numerous, clear, and able criticisms upon the subjects so abridged, which constitute part of the plan of the Retrospect, and are every where to be met with.

5. His deduction then follows: " Such," says he, " being " the state of the English Philosophical Journals, our readers " will not be surprized that we (Dr. T.) venture to offer our " claims to the attention of the public."

Whether it became Doctor Thompson to have assumed the office of Censor, with regard to the productions which he appears to consider as rivals; or whether it would have been more decorous for him, as a man offering himself in the venerable presence of the public, to have felt the consciousness of human infirmity, and expecting to have his own faults viewed with candour, to have avoided the volunteer task of exposing those of others. Into these points I do not enquire; and, if these had been the only objects of question, I should have been silent.

But I can enquire and can decide, that it did not become Dr. Thompson to endeavour to depress his rivals by stating or giving currency to untruths. This is a point of moral character which I will treat in no other way than by shewing he has done so. My statements are numbered correspondently with the paragraphs at the beginning of this Notice.

1. The Philosophical Journal was published for the first year, 1787, for the joint account of myself and Messrs. Robinsons. In 1788 the entire Copy-right became mine and has continued so without interruption ever since. No bookseller ever had power to employ, or did employ any person in editing or interfering with the copy of the Journal. That copy has been provided by myself and Correspondents; and I have always had one assistant, fully acquainted with the Sciences, and Languages, of my own knowledge and appointment, and not employed by a Bookseller. My name as Author and Proprietor has every month been before the Public: was it not the duty of a good man, instead of sheltering himself under the words, " if Report says true," to have enquired whether the Report was or was not true, before he ventured to join in propagating it?

2. I am informed that Mr. Tilloch (not Tulloch) was not a *printer* at Glasgow, and is *proprietor* (not publisher) of the Star. These are unimportant sounds in themselves; but they shew the disposition of Thompson to lower his supposed opponents, and his want of accuracy and correctness. All the world knows how many eminent men have been printers, and how little in a nation like ours, the science and acquisitions of men, depend upon their pursuits in business.

3. His prejudiced notice of the Repertory speaks for itself:

4. And so does his positive assertion that the Repertory is *merely* an abridgment. He could not but have seen, though he has thought fit to deny, the excellent original discussions it contains.

5. His summary deduction points out the spirit and motives of his statements; namely, to shew that the English Journals are bad, and, by inference, that his own will be much superior.

My principal object has been to expose the Doctor's conduct with regard to myself. The world must determine for him, whether that conduct can promote any interest for which a well-disposed mind ought to be solicitous.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1813.

ARTICLE I.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. BERZELIUS.

(*Continued from p.* 146.)

IN the explanation of these phenomena, there is a circumstance which confounds us more than all the others, viz. that there is only one body, that is to say oxigen, which possesses absolute and invariable electro-chemical characters. All the others, while they manifest a fixed and determinate relation with regard to oxigen, vary with regard to each other. Sulphur, for example, is positive with regard to oxigen, but is negative with regard to the metals; arsenic is positive with regard to oxigen and to sulphur, but it is negative with regard to the other metals; silver is positive with regard to oxigen, sulphur, and arsenic, but is negative with regard to most of the metals.

Oxigen alone possesses absolute and invariable electro-chemical characters.

[The author here subjoins the following annotation, which, on account of its extent, I have printed in the body of the page.—W. N.]

Though it may, perhaps, be too early for us to adopt any notions respecting this difficult subject, I shall here offer a conjecture

Theory of electro-chemistry.

jecture upon the manner according to which the whole of the effects may take place, without contradicting any of the results we possess concerning electricity.

Atoms possess electrical polarity; on the intensity of which their affinities depend.

One pole may be stronger than the other. The body is called electro-positive, or electro-negative, from its predominating pole.

Heat augments these electricities.

Admitting, that bodies consist of particles or atoms placed near each other, in such manner as may appear from their property of combining in proportions of their multiples, we may consider these atoms as possessing an electrical polarity upon the intensity of which the force of their affinity depends. In this case the chemical affinity becomes identified with electricity, or rather the electric polarity. In order to explain the different electro-chemical characters, we must add to the general polarity a kind of specific unipolarity, by means of which one of the poles contains more of the + E, or of the − E, than the opposite electricity in the other pole is capable of saturating. A body of which the positive pole predominates, that is, which contains an excess of positive electricity, constitutes an electro-positive body, and vice versâ. Many bodies require an elevation of temperature to enable them to act upon each other. It appears, therefore, that heat possesses the property of augmenting the polarity of these bodies; and that the difference in activity of the affinity at different temperatures, appears to depend on the same cause, in like manner as the force with which a combination preserves its existence, appears to depend on the intensity of the electric polarity when this is at its maximum, or rather the intensity of that polarity at the moment the combination is made. This circumstance explains why the phosphoric acid is decomposable by charcoal at an elevated temperature, although phosphorus decomposes the air of the atmosphere at a temperature at which charcoal has no influence upon that fluid.

The particles of homogeneous masses appear to unite like the plates of the electrophore.

In the theory of atoms, there is some difficulty in conceiving the difference between the juxta-portion of homogeneous particles, separable by mechanical means, and that of the heterogeneous particles, which produce a new particle, very seldom decomposable by means purely mechanical. The hypothesis of polarized atoms assists us upon this occasion. The cohesion of homogeneous particles may be compared to the juxta-position which we observe in the electrophore between the opposite electricities of the metallic plate and the resinous surface Contact keeps them in a state of charge or neutralization;

which, in fact, is simply juxta-position, and is destroyed when the surfaces are separated, and each appears again in possession of its original electric state. When heterogeneous atoms combine (whether the combination do consist simply in juxta-position, or, which is more difficult to comprehend, in a partial or total penetration) they appear to adjust or dispose themselves so as to touch with the opposite poles; of which the electricities produce a discharge which causes the phenomenon of elevation of temperature, almost constantly apparent at the time of any chemical combination, and the particles remain combined until their discharged poles are, by some means or other, restored to their former electric state.

Heterogeneous particles seem to unite by a discharge of electricity with production of heat.

As we know, from fact and experience, that bodies of the same electro-chemical class (that is to say, bodies in which we conceive that the same pole predominates) can combine, it appears, that the force of affinity depends rather on the intensity of the general polarity, than of the specific unipolarity; and from this reason it may be, that sulphur has more affinity with oxigen, than gold or platina has, although sulphur has the same unipolarity as oxigen, and those metals have an opposite unipolarity to that of oxigen.

The affinity depends more upon the intensity of the general polarity, than upon the excess in one pole beyond the other.

It is clear, that when two bodies, in which the same pole predominates, combine together, the new particle must possess their unipolar force concentrated in one of its poles, and must, consequently, have electro-chemical properties more intense; and this is a good reason why sulphur and oxigen produce the strongest acid. On the contrary, when particles possessing an opposite polarity unite, the polarity of one of the particles most frequently predominates; for example, in potash, and in most of the metallic oxides, the predominating pole of the metal also predominates in the compound. In some instances, the product is a neutral compound, in which neither of the poles predominate, such as the superoxides; in other instances, the pole of the metal predominates in one degree of oxidation, and that of the oxigen in another.

When bodies, having the same predominating pole, combine, the electro chemical property of the compound will be more intense; and vice versâ.

The combination of polarized atoms requires a motion to turn the opposite poles to each other; and to this circumstance is owing the facility with which combination takes place when one of the two bodies is in the liquid state, or where both are in that state; and the extreme difficulty, or nearly impossibility,

Polarized atoms must turn to each other in order to combine. This will be easily done if one or both be fluid. Solids can scarcely combine. The atoms of gases have less electric action because too remote.

of effecting an union between bodies, both of which are solid. And again, since each polarized particle must have an electric atmosphere, and as this atmosphere is the predisposing cause of combination, as we have seen, it follows, that the particles cannot act but at certain distances, proportioned to the intensity of their polarity; and hence it is that bodies, which have affinity for each other, always combine nearly on the instant when mixed in the liquid state, but less easily in the gaseous state, and the union ceases to be possible under a certain degree of dilatation of the gasses, as we know by the experiments of Grothuss, that a mixture of oxigen and hidrogen in due proportions, when rarefied to a certain degree, cannot be set on fire at any temperature whatever.

The pile restores the atoms to their former state.

The chemical action effected by the discharge of the pile, consists in the particles in a combination being re-polarized. In a combination of particles having the same unipolarity, the pile merely restores, by the decomposition, the general polarity, because their specific unipolarity was not changed by their union; but in combinations of opposite unipolarity, it likewise restores the specific unipolarity of the elements. May we conclude, that, in the first case, the general re-polarization takes place in the same manner as the loadstone gives magnetism to a small particle of steel, and that in the second, the pile contributes, by its own specific energies, to restore the predominating poles.*

[Here the annotation concludes.—W. N.]

Classification of bodies as to their disposition to be collected round the poles of the pile. Elc[illegible]

In my essay upon chemical nomenclature, I have divided bodies into electro-positive and electro-negative, the first of these denominations being appropriated to bodies which, by the action of the pile, are collected round the positive pole; and vice versâ, I have noticed the probability, that these names had been employed in the opposite sense; and my subsequent reflections upon this object, obliges me, at present, to change these denominations for each other. I shall, therefore, hereafter call those bodies *electro-positive* which are collected round the negative pole, and those *electro-negative* which are collected round the positive pole. With regard to the electro-chemical relations of bodies mutually, I shall divide them into five different classes.

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* In this beautiful generalization of facts, which promises to become more conclusive the more it shall be studied, this last paragraph seems rather obscure. The poles of the voltaic pile appear to present, to the principles of a compound, points of attraction more powerful than that which maintained the combination; and they transmit the electric energy from particle to particle, so as to complete the total decomposition. But we do not yet appear to possess analogies to carry us much farther.—N.

positive attach to the negative, and *electro-negative* to the positive pole.

1. *Absolutely electro-negative*; oxigen alone.

1. Absolutely electro-negative.

2. *Electro-negative in general*; all combustible bodies which produce acids with oxigen, are constantly collected at the positive pole of the pile. To this class the metalloids belong, and among the metals arsenic, molybdena, and wolfram.

Electro-ne[illegible]ive.

3. *Bodies of a variable electro-chemical nature.* This class includes (*a*) such bodies as,' when combined with oxigen, are electro-positive with regard to the preceding class, but electro-negative with regard to the bodies which constitute the last of the subsequent classes; (*b*) such bodies as, in one degree of oxidation, constitute a saline base, and in another degree an acid. Tellurium is an example of the first, and antimony of the latter.

3. Variable.

4. *Indifferent.* Oxided bodies, which possess no decided character, being neither acid nor saline bases. Such are the oxides of tantalium and of silicium. This class likewise includes the combinations of acids with saline bases, that is to say, the salts.

4. Indifferent

5. *Electro-positive.* Combustible bodies and their oxides, which, during the action of the pile, are never collected round the positive pole, and of which a great part, when combined with oxigen in excess, instead of forming acids, produce superoxides. Such are potassium, baritium, lead, silver, &c.

5. Electro-positive.

It is proved by experiment, that the more opposite the electro-chemical nature of two bodies is, the stronger in general i[illegible] their mutual affinity. A combustible body consequently ten[illegible] with greater force, to combine with oxigen, than with any other combustible body with which it may have affinity. Hence we may conclude, that, if it were possible to obtain pure oxigen in the solid form, and if, in that state, it were put into contact with a combustible body, it would become much more strongly electric than, for instance, sulphur with copper, and would, in fact, produce, in combining with the combustible body,

Mutual affinity being [illegible]nger, the [illegible]e opposite the electro-chemical nature of bodies. Combustibles will unite strongly with oxigen,—and produce elevation of temperature

by a process resembling the voltaic discharge:

body, an elevation of temperature much higher than could be produced by the combination of any other body with the same combustible. These reflections appear to indicate that, in the phenomenon of combustion, as in general in every chemical combination, the phenomenon of fire is produced by a cause analogous to that which is manifested on the occasion of the discharge of the electric pile; that is to say, by a discharge between the opposite electricities of the oxigen and of the combustible body, which is made at the moment of combination.

and this will depend m[illegible] on the streng[illegible] of affinity, than on change of density, &c.

The same considerations also explain why the phenomenon of fire is more intense accordingly as the affinity of the bodies which combine is more powerful (varying from the slightest elevation of temperature to the most intense fire) without any remarkable relation between the expansion or condensation the bodies may have undergone from their union.

Hence the effects of sulphuration are similar to those of combustion:

This electro-chemical view explains what was so difficult to be comprehended in the time of our predecessors, namely, how sulphuration could produce a phenomenon of fire exactly similar to that produced by combustion; and it classes together all the disengagements of caloric or fire, occasioned by chemical combinations. As it explains, in a consistent manner, that which the old theory could not account for, it appears to deserve our confidence, or at least our attention. I shall explain my notions by an example.

And charcoal between the poles of the pile in hidrogen or azote, is strongly ignited as in oxigen; but there is no combustion.

If any very powerful electric pile be discharged by pieces of charcoal in hidrogen or azote gas, we see the charcoal become ignited, and produce the same phenomenon, as if it were actually burning. A spectator, who, on this occasion, had no knowledge of the influence of the pile, would say that the charcoal was burning. But, nevertheless, there is, in this case, neither oxidation nor chemical combination of any ponderable matter with the charcoal, and, notwithstanding this, the phenomenon of fire is the same as if it had been produced by combustion. Now, it appears to be a well-founded conclusion, that the same effects are produced by the same causes; that is to say, that the fire in each of these cases is produced by an electric discharge.

The oxigen is not *condensed* by burning charcoal; and,

Charcoal does not condense oxigen by burning, but, on the contrary, is dissolved in the gas of which the volume undergoes no change. We cannot, therefore, assert, that the caloric of combustion of the charcoal is the effect of a condensation, namely, that the oxigen gas has parted with the caloric which was employed in maintaining its gaseous form; and it is clear, that the fire owes its origin to some other circumstance. Those who may not be disposed to approve the electro-chemical explanation, may, observe, that the fire in this combustion is produced by the difference between the specific heats of oxigen gas, and carbonic acid gas. But, although it cannot be denied, that such a cause (or incident) may contribute to (or accompany) the production of heat, it can be easily shewn, that it is not the principal or general cause; because the nitric acid in which the oxigen still preserves its property of producing fire with a number of combustible bodies, possesses as little specific heat as the carbonic and the sulphuric acids. In like manner, the difference of specific heat between the metallic sulphurets and that of a metallic body, is too inconsiderable to afford a plausible reason for the fire produced by sulphuration.

consequently the heat is not caused by condensation,

nor by change of capacity.

Electro-chemical cause of heat, in simple elective affinity, elucidated.

When a combination already formed, as, for instance, between A and B, is decomposed by the more powerful affinity of a third body C, so that this last separates A from the combination AB, and forms CB—such a decomposition is usually accompanied with an elevation of temperature, or even with fire; and this elevation is greater the more considerable the difference may be between the affinities of A and of C to B. We may form a notion, that this effect is owing to a more perfect neutralization of the electro-chemical properties of the constituent parts in the new, than in the old combination. If, on this occasion, B were oxigen, and A and C two combustible bodies, the electro-chemical nature of B must be admitted as more perfectly neutralized by C than by A; and at the instant when A is reduced to its original combustible state, it receives from C, which loses its like state, a quantity of positive electricity, equal to what it had lost when it entered into combination with B.

The properties of compounds are remarkably affected by the electric nature of their components, e. g sulphur

When bodies combine with others, in some instances more positive, and in others more negative, than themselves, are found after these two circumstances in very different states; as sulphur, for instance, is in a quite different state in the sulphuric acid, from that which it possesses in the sulphuret of lead. From the former it can be disengaged by a number of electropositive

bodies,

and oxigen, which are both neg. form sulph. acid and the sulphur may be disengaged by many el positive bodies.—but in sulphuret of lead no el. pos. body can take the lead, but an e. neg. body, viz. oxigen is required.

Cause why sulphur and phos. are rendered more combustible by union with alk. The latter is el. pos. and renders the sulph less neg. and therefore more disposed to oxigen.

bodies, but from the last it cannot be disengaged by the affinity of any electropositive body to the lead; but for that purpose the affinity of another body, more electronegative than itself, namely oxigen will be required. Sulphur has, therefore, occasion for opposite electricities, in order to effect its separation from these two different combinations. It is well deserving of attention, that when such an electronegative combustible is combined with an electropositive oxide, the combustibility of the former (or its electropositive relation as to oxigen) is considerably increased; probably because its electronegative dispositions have been destroyed by the positive electricity of the oxide. We observe this in the great oxidability of sulphur and of phosphorus, combined with the alkalis, or alkaline earths. In a combination of two combustible bodies of opposite electrochemical natures, this augmentation of combustibility does not take place, and the combination of the two is less combustible than that one of the constituents, which was the most so, because one of them has lost exactly as much of its electropositive characters, as the other (the electronegative) has lost of its characters; and as in this case the effect must result from the sum of the affinities, it follows that the affinity of the most considerable is diminished in proportion as the quantity of the other is greater, and its affinity for oxigen less. It is from this explanation that we may conceive a phenomenon of which I shall give an account; namely, that the oxide of tin mixed with the oxide of gold, becomes reduced to the metallic state without the addition of a more combustible body; simply by the action of heat, by forming a metallic alloy of gold and tin, which is not decomposable by fire, even when fused with saltpetre.

An opposite fact.

Heat discomposes bodies; whence probably caloric may become electricity and restore the orig. properties of the particles.

Heat often produces, without the co-operation of other circumstances, a decomposition of combinations; and as in the electrochemical theory we form the conclusion, that no body can be restored to its original properties without the influence of the same electricity which it parted with when it entered into combination, we must likewise imagine as a consistent consequence of this fact, that in the same manner as the separate electricities, by their combination disappear and produce fire, so caloric in its turn, when accumulated and tending to regain its equilibrium, is capable, in certain circumstances, of disappearing

Remontoire Escapement by Mr G. Prior Junr.

Fig 1.

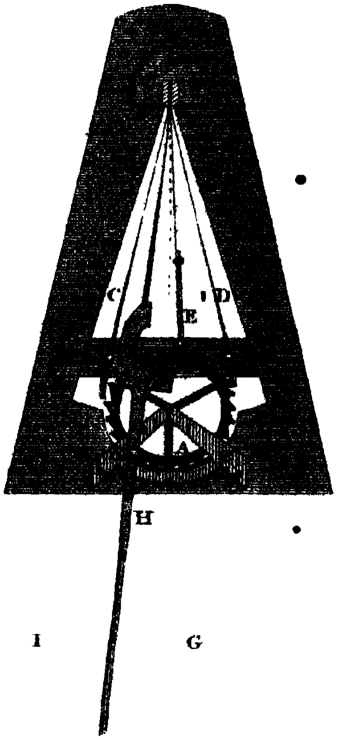


Fig. 2.

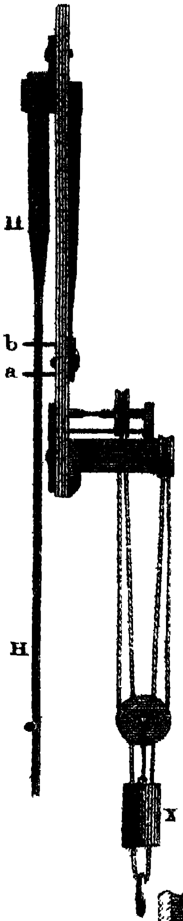
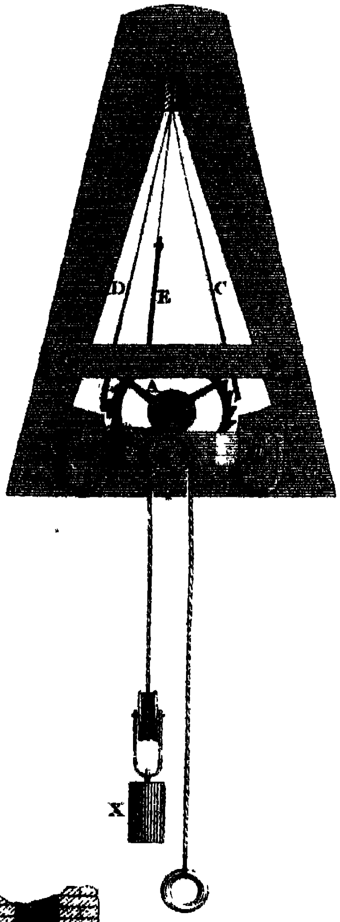
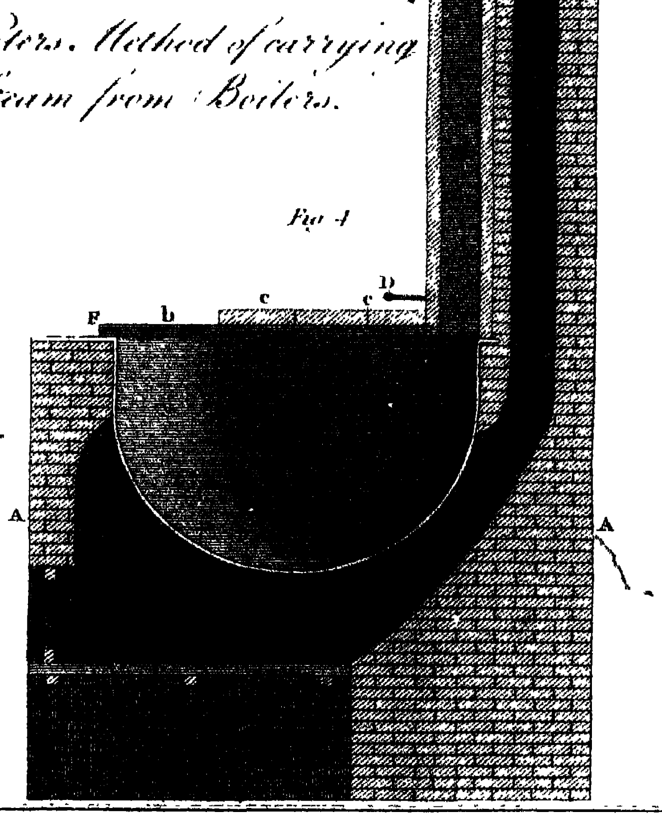


Fig. 3.



Mr Webster's Method of carrying off Steam from Boilers.

Fig 4



disappearing as caloric, and re-appearing in the electric state restored to the separate elements.

The phenomena of the pile are not caused by affinity?

If the chemical affinity be nothing more than the result of the polarity of the particles, it will follow decidedly, that it cannot be affinity which is the first mover, and causes the electric phenomena; but that on the contrary, the play of chemical affinities in the pile must be a consequence of these last: And this opinion is accordingly confirmed by experiment.*

[Annotation. See remark at the note on page 154.]

Experiment to shew that the ordinary chemical actions of bodies do not produce that phenomena.

* I was long of opinion that the oxidation of the zinc in the electric pile, was the cause of the change of electricity, and I endeavoured to prove that this hypothesis was sufficient to explain the phenomena of the pile (See my Theory of the Electric Pile in the "Neues Allgemeines Journal der Chemie," by Gehlen, in the year 1807.) But the experiments of Davy and Pfaff, having rendered my opinions less probable, I endeavoured to convince myself of the truth, by an experiment which I think decisive. I took 12 tubes of glass, half an inch in diameter and three inches in height, and closed at one end, I half filled them with a strong solution of the submuriate of lime (such as is obtained by the residue after the preparation of caustic ammonia) and above this fluid I poured diluted nitric acid, with the precaution not to mix the liquids. I ranged these tubes in succession, and then took copper wires, round one of the extremities of each of which I had melted zinc, in order to attach a knob of that metal to that end. I immersed the zinc-coated ends of each into one of the tubes to the bottom of the submuriat, and then bended the upper ends of the respective wires, so as to immerse them into the middle of the acid of each nearest tube. This arrangement, consequently, formed a pile in the order following: copper, zinc, submuriate of lime, nitric acid; copper, zinc, &c. It is evident that the chemical affinity which produces oxidation at the common temperature, was here at the surface of that part of the copper, which was in contact with the nitric acid; and that if this oxidation had been the primary cause of the electricity of the pile, the *pole of copper* in this construction *ought to have possessed the same electricity* (namely the positive) *as the zinc pole in the common pile.* Before the poles, or extremes, of this small pile were connected, the copper continued to be constantly dissolved in the acid,

By a row of tubes a pile was made in which the order was copper, zinc, submuriate of lime, nitric acid; copper, &c. While the extreme poles were unconnected, the nitric acid dissolved the copper, and the submuriate had no effect on the zinc. If oxidation were the cause of positive el. the copper would here have been pos, acid, which it turned blue, and the surface of the zinc remained metallic and without any perceptible change. And lastly, I combined the poles by means of silver wires, passed into a tube filled with a solution of muriate of soda. But I was greatly surprised to find the effect *directly contrary* to what the theory which considers oxidation as the cause of the electricity of the pile had led me to expect. The solution of the copper instantly ceased and the zinc became covered with a mass of white oxide, vegetating on all sides in the form of wool. The pole of the copper produced hydrogen gas as usual, and the zinc pole caused an abundant precipitate of muriate of silver. The electric state, therefore, produced in this case an affinity, which, at the ordinary temperature of the atmosphere is inactive, and caused another very active affinity to cease, which was already in operation; and this could be effected by no other cause than that of the electricity produced by contact, which occasions the electric charge of the pile, and disposes the affinities which shall be put into activity. This little electric pile, was very powerful, and disengaged so large a quantity of gas, as would not have been exceeded by 100 pair of plates. But what could be the cause of this?—I exchanged the submuriate for neutral muriate; it then produced a very moderate effect, corresponding with the number of pairs; and lastly, I substituted neutral muriate of zinc instead of the muriate of lime, and then the effect was scarcely perceptible, though it continued sufficient to prevent the oxidation of the copper in the nitric acid, and to shew that the conductor of the zinc pole continued always to be oxided. It appears, therefore, that the activity of the pile depends on the liquid substance, which during the process must change place; and that the most advantageous construction of a pile is copper, zinc, alkaline substance, acid, copper, zinc, &c. The pile will continue active until the order becomes inverted, that is to say, copper, zinc, acid, alkali, &c. This experiment also proves how necessary it is in every theory of the pile to attend to the chemical effect which must take place in the liquid.

But when the extreme poles were joined, *the action in the tubes was instantly reversed.* The zinc was then oxided, the copper ceased to be dissolved and the pole on the zinc side was positive as usual,

the elect. was not therefore governed by the action of the ordinary simple affinity.

Changes in the liquids.

[Here the Annotation ends.]

The electricity excited in the pile operates by affinity to great distances.

The connection between chemical affinity and the electric state, is also such, that upon every occasion wherein the effect of the electricities, excited by contact, cannot take place, the affinity acts only at a distance, infinitely small and impossible to be determined; but on the contrary, whenever that effect can manifest itself, it acts to very considerable distances, as for instance, in the precipitation of metals upon each other; and it is very probable, that actions at a distance produced by the electric powers do take place in the bowels of the earth, and contribute not only to the great revolutions of which we find the astonishing vestiges, but likewise to the tranquil formation and decomposition of minerals.

The electro-chemical properties shew why oxigen produces with some bodies acids, and with others alkalis; and that it ought not to be called the acidifying principle.

From the electrochemical view of nature we also derive a correction in our notions concerning the principles of acidity. The celebrated Lavoisier having found that sulphur, phosphorus, charcoal, arsenic, &c. produce acids when combined with oxigen, considered oxigen as the acidifying body. But notwithstanding this conclusion is supported by the circumstance that oxigen is the most electronegative of bodies, and the acids are also electronegative bodies, bodies were afterwards discovered, possessing the principal characters of acids without containing oxigen; and after the discovery that the saline bases are also oxided bodies as well as the acids, it would be equally incorrect to attribute the acid characters to oxigen, or some of its combinations, as to suppose it to be the principle of basidity, or to call it the alkaligenous principle.

and from these and other facts, it is shewn, that it depends on the nature of the radical and not upon oxigen, whether the oxide shall be an acid or a base.

The electro-chemical explanation in which the combustible radical of an acid is already in its non-oxided state, electronegative towards the radicals of the salifiable bases, being considered along with the experimental determination, that the radicals of the bases, and of the acids, very often combine in the same proportions in the combustible state, as in the oxided state, proves that it is to the nature of the radicals, and not to oxigen, that we ought to attribute the nature of the product of oxidation. In this manner it is that sulphur and potassium combine for the most part in the same proportions, in the sulphuret of potassium, in the hydrosulphuret of potassium, in the sulphuret of potash, in the hydrosulphuret of potash, in the sulphite, and in the sulphate of potash; and it is by no means difficult to observe, that sulphur not oxided, performs the part of an acid, that is to say, of an electronegative constituent in the sulphuret and the hydrosulphuret of potash. These observations upon sulphur and potassium may be applied to all the other combustible radicals which possess an opposite electric nature. And from all this it follows that *it is the radical itself and not the oxigen which determines whether the oxide shall be an acid or a base.*

Whether the electricities and caloric be matter.

A great question still remains to be discussed; *Whether the electricities and caloric be matter or merely phenomena?* This question has long been disputed, and will long continue in dispute before it shall be decided; which, perhaps, will never be done. At present we must content ourselves with reasoning, though our arguments can at best be considered as the sport of imagination upon interesting objects.

Though they do not exhibit gravitation, nor aggregation,

yet they may be matter.

If, by the word matter, we understand a body which manifests its presence by gravitation, which possesses a certain kind of aggregation, and fills the place of its existence, in such a manner as to exclude all other bodies—it will certainly follow, that these problematic beings are not matter. But is it not possible that they should be matter, without possessing these characters; or are the reasons greater for considering them as phenomena?—A number of philosophers have considered light as the oscillations in a problematical matter produced by luminous bodies; and this hypothesis owes its origin to the analogy which exists between sound and light. But this oscillating matter has not yet been discovered by chemistry; and consequently the hypothesis itself cannot be satisfactory, because it presupposes a thing of which we cannot find the existence. But if we even admit that light, and the mechanical phenomena which are presented in its motion, can be attributed to a vibration analogous to that which constitutes sound, this mechanical motion cannot produce the chemical effects of light; such as the alterations in the form, the aggregation, or the composition of bodies; more especially as we have never discovered that sound could produce any such effects. There is, consequently, some probability that caloric may be matter, and that light and all radiations may consist in modes of propagating that matter.

The hypothesis that light is oscillation objectionable; because, the oscillating matter is unknown; and mere oscillation will not explain the chemical changes.

Matter may have chemical affinity and not gravitation,

It may be demanded whether we can imagine the existence of a matter possessing chemical affinities without obeying the laws of gravitation. There is certainly no contradiction in this position. We admit the difference between cohesion and gravitation, and we are also led to distinguish the latter from chemical affinity. Some philosophers have sought to prove that caloric possesses weight, though too small to be perceived; but if caloric be even supposed to be matter, it is not probable that it should possess weight, because the property of radiating excludes all the effect of gravitation, and because this matter, if heavy, ought to accumulate without limit in the planetary bodies and at length destroy them.

Caloric does not gravitate.

From the relation which exists between caloric and the electricities, it is clear that what may be true with regard to the materiality of one of them, must also be true with regard to that of the other. There are, however, a quantity of phenomena produced by electricity, which do not admit of explanation, without admitting at the same time that electricity is matter. Electricity, for instance, very often detaches every thing which covers the surface of those bodies which conduct it. It, indeed, passes through conductors without leaving any trace of its passage; but it penetrates non-conductors which oppose its course, and makes a perforation precisely of the same discription as would have been made by some thing which had need of place for its passage. We often observe this when electric jars are broken by an over-charge, or when the electric shock is passed through a number of cards, &c.

If electricity be matter, it is concluded that caloric is also matter:

But electricity detaches the coverings of conductors;

and perforates bodies:

We may, therefore, at least with some probability, imagine caloric and the electricities to be matter destitute of gravitation, but possessing affinity to gravitating bodies. When they are not confined by these affinities, they tend to place themselves in equilibrium in the universe. The suns destroy at every moment this equilibrium, and they send the re-united electricities in the form of luminous rays towards the planetary bodies, upon the surface of which, the rays being arrested, manifest themselves as caloric; and this last in its turn, during the time required to replace it in equilibrio in the universe, supports the chemical activity of organic and inorganic nature. If we can imagine all this to be possible, we possess a notion how the sun can cause a body to emanate from itself without loss of its own volume, and without this emanated body producing on the bodies which arrest it the effects of a gravitating and falling matter.

Whence it appears probable that caloric and the electricities are matter, &c:

But it is proper to put an end to these conjectures. I hope

Apology for

that

conjectural speculations.

that the necessity of referring to some electro-chemical theory will excuse the attempt of having imagined one; and though this necessity cannot be pleaded in justification of extravagant conjectures, they will, perhaps, be thought pardonable in a department of science where experiment is yet wanting to regulate the efforts of imagination, although such efforts may be useful to arrange the existing facts, and indicate the course which may lead to the discovery of new ones.

(To be continued.)

II.

Notice respecting Experiments on the Freezing of Alcohol, by Mr. HUTTON*.

The author's motives for publication.

I HAVE been prevailed upon to communicate a notice of some experiments and observations I have made on the production of a great degree of cold. It is scarcely necessary to observe, that my doing so at this time is not a matter of choice: these experiments and observations were mentioned to my friends, as they were made without any injunction as to secrecy, as I did not anticipate that such communications would either be received with so much avidity, or repeated with so much eagerness. The consequence has been, that accounts of these experiments have now got into very general circulation, and many very contrary and erroneous ideas have been entertained, not only as to their extent, but even as to their nature; and it has been imagined, that a communication like the present is the only way to obviate these misconceptions —misconceptions which I owe as much to you as to myself to remove.

Advantages to be derived by change of form in bodies from cooling.

The importance of a method of producing a great degree of cold becomes apparent, when it is considered, that it is at present a very common opinion among chemists,—an opinion founded on a very general analogy, that all gases may be reduced to the state of liquids by the abstraction of caloric, and that by a farther abstraction of caloric all liquids in their turn, may be reduced to the solid state. If this be true, and we were in possession of a method of sufficiently abstracting caloric, all bodies whatever might be reduced to the solid state. We should thus become acquainted with a great number of substances that we have hitherto had no opportunity of examining; many powerful agents would likely be obtained; many new and interesting compounds formed, and much light could not fail to be thrown on the constitution of known substances.

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* Read to the Edinburgh Institute, on the 2d of Feb. last.

Mention and date of a method for producing extreme cold.

Directing my attention to this subject, in the summer of 1810, a method occurred to me, by which I imagined a greater degree of cold might be produced than had hitherto been obtained. Although the power of this method appeared in theory almost indefinite, yet it was easy to foresee that in practice many circumstances might at first concur to set limits to its application; from the nature of these circumstances, however, it was to be expected, that some of them might be considerably modified, and many of them might in time be altogether removed; and thus the practice made, in some degree, to approximate to the theory.

At the time this method occurred to me, the pressure of my professional avocations did not allow me to prosecute it; but, as I anticipated some leisure in the following autumn, I immediately began to provide, at any leisure moments I had, such apparatus as I considered absolutely necessary, or was most likely to be useful. The little dependence, however, which is to be placed on general reasoning on such subjects, and the apprehension that the method might have been previously tried, and found insufficient by others, prevented me from providing any very extensive apparatus.

A thermometer of alcohol (sp. gr. 798)

My first experiment was tried in the following autumn. The thermometer was filled and sealed by myself. The tube was previously tried by the common method, and found, as nearly as such tubes are commonly to be met with, of equal calibre throughout. The spirit with which it was filled was prepared by Richter's process, and afterwards re-distilled by itself. Its specific gravity at 62° was 798. The points 60° and 100° were determined by a mercurial thermometer, which had been made with the usual precautions; the interval was divided into four spaces, each of which, of course, correspond to 10°; the part of the stem below 60° measured nearly 18 of these spaces. A mark was made at every space, till, on arriving at the end of the 17th, the graduation could not be carried farther. This point, of course, corresponded to + 60° − 170° = − 110 deg. of Fahrenheit's scale.

was exposed to the cold by this method, and the alcohol partly frozen.

This thermometer was exposed to the cold produced by the method alluded to, and after some time was examined, when the alcohol was found to have passed all the marks, and was obviously sunk within the ball of the thermometer. A slight degree of discoloration was observable. The thermometer was replaced, and examined about five minutes afterwards, when the ball of the thermometer was found broken, and crystals adhered to the fragments.

Other portions of the alcohol completely frozen in a tube.

I next took a glass tube, about 3-10ths of an inch in diameter, and sealed at one end; into this I poured alcohol till it stood in the tube 4-10ths of an inch deep, and then exposed it to the cold, produced as before; after some time it was so completely solid, that on inverting the tube it did not drop, and only a very minute stream was perceived to glide slowly down the inside of the tube; when this stream had reached nearly the middle of the tube, the whole suddenly fell out, and, pitching in a glass, was broken into several pieces, which quickly melted.

This experiment was several times repeated, but by allowing the alcohol to remain a little longer exposed to the cold, it became so completely solid, that on inverting the tube, not the least portion of fluid could be perceived to separate from the mass.

In order to be as certain as possible of the strength of the alcohol I employed, I again took its specific gravity, and the result corresponded with what I before obtained.

These experiments, therefore, left me no room to doubt that I had frozen alcohol, which, at the temperature of 62°, is of the specific gravity 798.

Repetitions of the experiment.

Being appointed to deliver the course of lectures on chemistry for the session 1810-11, I had no leisure, at that time, to pursue these experiments. They were resumed, however, in the autumn of 1811. The second experiment was repeated and varied, and solid masses of alcohol of some magnitude obtained.

tained. Some of these I soldered together, using as a hot bolt, a rod of frozen mercury, and sometimes a straw cooled down to a very low temperature.

It now appeared to me to be an object of some importance to ascertain the form of the crystals which this substance assumes. This I found attended with some difficulties, which I did not anticipate, and attempts to overcome them have led to the discovery of some facts which I did not at all expect.

The alcohol separated into three distinct strata.

The common masses exhibited crystals of different forms; two kinds appeared to predominate, and each was tolerably distinct in its kind; but it was not very easy to perceive by what increments or decrements the one could be supposed to pass into the other; a rather casual circumstance, however, explained the source of this variety. Attempting to freeze alcohol by a modification of the general process, which I conjectured would yield more regular crystals than the common method, I observed, that before crystallizing, the alcohol separated into three very distinct strata; the uppermost was of a pale, yellowish green, while the second was of a very pale yellow colour; both these strata were very thin; the last mentioned was rather the thickest; the lowermost stratum was nearly transparent and colourless, and very greatly exceeded the other two in quantity. After allowing a part of the lower stratum, which I conceived to be the pure alcohol, to freeze, I attempted to pour out the remainder; but was prevented by the upper strata, which proved to be solidified. The lowermost of these two strata bore some marks of crystallization; the upper had none, and proved so firm, as to resist a straw with which I attempted to perforate it, to open a passage for the sublatent liquid. On removing part of these superior strata, and decanting the remaining fluid, the crystals of the lower stratum appeared very distinctly to be rectangular prisms of equal planes, a few of them on one side of the glass surmounted by quadrangular pyramids, but most of them by dihedral summits. This experiment I repeated several times, and the results coincided.

The lower stratum, or greater part, gave rectangular prismatic crystals.

The alcohol, when fused, had suffered no change.

In order to ascertain whether these phenomena arose from a decomposition of the alcohol, or from the separation of foreign substances previously held by it in solution, the products of several of these experiments were mingled together in a stoppered matrass; the whole was then raised to the temperature of about 120 deg. by a water bath of that temperature. The substances forming the different strata united together, and formed a colourless liquor, which had the specific gravity, and all the other properties of the alcohol from which it was obtained. This experiment was repeated several times, and the results were uniform, affording sufficient evidence, that the alcohol had not been decomposed by this process, but that the superior strata consisted of foreign substances, which it had held in solution. The variety in the form of the crystals obtained by former experiments, was, therefore, most likely occasioned by the presence of these foreign substances, a phenomenon not uncommon in chemistry.

The result of these experiments led me now to perceive, that the assumption that alcohol, prepared by Richter's process, is perfectly pure, or at most contains only a very minute portion of water, is entirely gratuitous. The diluted alcohol of commerce, from which the more concentrated is obtained, is well known to contain different volatile impurities; and since Richter's process makes no provision for the separation of these, we ought rather to expect still to meet with some portion of them in alcohol prepared in this manner.

Properties of the three substances.

I next proceeded to examine the properties of the different substances into which I had separated Richter's alcohol; but the time I had now left for this purpose was too short for making much progress in this inquiry; a few only of their habitudes with water, and one with another, were all that I had time to examine; even these I could examine only imperfectly.

The lowermost stratum, or nearly colourless fluid, which I have called alcohol, had no flavour, and produced on the organ of smell only a sharp pungent sensation. It has the remarkable property of smoking when exposed to the air, and when diluted with water it differs considerably in taste from common diluted spirit of wine.

The pale yellow substance, or second stratum, has a pungent taste, leaving an impression of sweetness. It has a very strong but agreeable smell. When mixed with the alcohol, and diluted with water, it has very much the flavour of the better kinds of highland whisky. It readily dissolves in water, and communicates to that fluid its peculiar flavour.

The pale, yellowish green substance, which composes the uppermost stratum, has a strong and very offensive smell, and a very sharp nauseous taste. It dissolves in alcohol, to which it communicates its peculiar flavour ; its disagreeable smell is considerably heightened by this combination. It dissolves in water, though less readily than the substance last treated of. The compound, when much diluted and heated, has very much the flavour of the *low wine* of our lowland distillers, at the time it issues from the still.

The two last mentioned substances, or those of which the two upper strata are composed, when mixed together and greatly diluted with water, have very nearly the flavour of alcohol. They have rather more volatility than water ; for when half a solution of them has been distilled over, the distilled part has a much stronger smell than that which remains in the retort.

It may be proper to mention, that from the circumstance of my sense of smell having been for some time extremely obtuse, I have been under the necessity of trusting to others for the facts regarding the flavour of these new substances and mixtures ; from the uniformity of the reports, however, which I have received from different persons, I have no doubt that these facts are correct.

Alcohols of less strength frozen with similar results.

Besides that from which I filled the thermometer in the first experiment, I have operated on alcohol of the specific gravities 802, 797, and 784 ; the specific gravity of the last was taken when its temperature was 66 deg. and it is probably the most concentrated that has ever been obtained. But with alcohol of all these different strengths, the general results were similar. In alcohol obtained from different sources, the proportions of the impurities were different, both with regard to the pure alcohol, and to one another, but I have met with none that did not contain both.

From these experiments I think it is ascertained ;

Recapitulation.

1st. That the strongest alcohol which we are able to obtain, may be frozen by the method alluded to.

2d. That this alcohol contains at least two foreign substances, which are highly volatile, and, so far as is known, can only be separated by freezing.

3d. That it is to those substances that alcohol owes its peculiar flavour, and that, according as the one or other predominates, the flavour of the alcohol is agreeable, or otherwise.

Last autumn I resumed this subject, and my attention was chiefly directed to the habitudes of these impurities with the chemical re-agents. This I found attended with considerable difficulties, none of the least of which was to procure a sufficient quantity of these impurities in a separate state. The series of experiments I proposed to myself on this subject have not yet been completed; but I may remark, that the result of some of those I have made, promises to afford practical hints of considerable importance to those brewers whose products are intended to afford spirituous liquors.

From this notice it will be observed, that I have scarcely yet entered on the wide field of inquiry, for cultivation of which, the method alluded to appears to offer so powerful an instrument. Alcohol only has been subjected to experiment; it was the only liquid which had resisted all attempts to reduce it to the solid state by the abstraction of caloric. If these experiments be correct, we may now pronounce it a general law, to which there is no exception, that all liquids with which we are acquainted may be reduced to the solid state by a suitable abstraction of caloric. Whether all gases may be susceptible of reduction to the solid state, by abstraction of caloric, remains to be ascertained; although, as I have mentioned, analogy renders it in the highest degree probable.

The examination of the singular substances, which alcohol prepared by Ritcher's process contains, has drawn me aside from the course of experiments I prescribed to myself, and taken up that time which I intended to have devoted to the examination of the effects of cold on the gaseous bodies. Whether I shall proceed to these bodies, or resume the examination of the habitudes of the alcoholic impurities with the re-agents, will much depend on the leisure which I can obtain; but to whichever of them I may direct my attention, I shall not fail to give the earliest information of the result to the Institute.

Annotation.—W. N.

Remark upon As Mr. Hutton's experiments and observations, and perhaps more or less of his method, were communicated to his friends, it is to be regretted that he has not described it in this notice; which would, at least, have secured him against the pretensions of those who, from conjecture or otherwise, might perform the same. Without departing from the respect due to an inventor, I consider it to be quite allowable for me to make a few remarks in this place, for the gratification of such of my readers as may not be familiar with the general subject.

inventions, announced, but not described.

If we except the direct cooling process, by communication with bodies at a lower temperature, and the few instances, if any, wherein cold can be said to be produced by chemical union, without change as to the state of aggregation, we can look to no other means of depressing the temperature of bodies, within our knowledge, but such as may be founded upon their augmentation of capacity for heat, when they pass from the solid to the fluid, or from the fluid to the gaseous, state. In *the first* of these two methods, certain bodies, such as snow and salt, one at least being in the solid state, are mixed and combine; and if the combination be not congealable at (or its freezing point be lower than) the heat of the surrounding or neighbouring bodies, the compound will be fluid, and will take from those bodies all that heat which its increased capacity as a fluid demands, for the maintenance of that state; and consequently those bodies will be cooled,—and one limit of this process will be at the freezing point of the compound, below which it cannot go; though from the heat of the surrounding bodies, it may be prevented from arriving at that point.

Except by mere communication, and perhaps combination, there are no cooling processes known, but what arise from fusion or gazification. The *first* is by freezing mixture,

But many of the freezing mixtures, at present known, seem to have their point of congelation far beneath any temperature we can practically look to; and, therefore, a very considerable part of the process of cooling by means of them has been directed to the prevention of the effect of foreign heat, by first cooling the ingredients, and surrounding the vessels with other cooling materials. Whether these precautions have been as much varied and applied, as the circumstances appear to demand, may, with justice, be doubted.

which in practice can scarcely be limited, but by the surrounding heat.

In the *second* method, by the evaporation of a fluid, such as water in various economical processes, and alcohol and ether in philosophical experiments, the rapidity with which the gaseous state is assumed, under like circumstances, governs the result;

The *second* method is by evaporation;

result; and this rapidity will be prodigiously increased by keeping off the surrounding pressure, as in Professor Leslie's experiment. Whether there be any practical limit of temperature, below which these or all volatile or fluid bodies could be prevented from assuming the gaseous state, is, I think, beyond the reach of our inquiries.

and probably subject only to the same limit.

Freezing processes may be improved by discovery or selection of a freezing mixture, and by absorbing the extraneous heat, either by Walker's or Leslie's method.

These cursory remarks upon cooling processes, may lead us to infer, in the way of conjecture, that Mr. Hutton's process may consist in the discovery or use of one of the most powerful freezing mixtures, and preventing the influence of the surrounding heat by a judicious application of the means similar to those pursued by Walker;—or, much rather, that instead of this last, he may have applied Professor Leslie's process as to the external cooling, by evaporation of ether in vacuo, to a vessel containing his freezing mixture. The apparatus for doing this, or for effecting his purpose otherwise, would demand a display of skill which we may reasonably expect will add to the philosophical reputation of Mr. Hutton.

III.

Some Remarks on the Use of Nitrate of Silver, for the Detection of minute Portions of Arsenic. By ALEX. MARCET, *M. D. F. R. S.**

Test for detecting arsenic: viz solutions of ammonia, and of nitrate of silver, added by alternations.

IN the interesting account of the poisonous effects of arsenic, presented to the Society by Dr. Roget, and published in the second volume of the Medico-Chirurgical Transactions†, the author has recommended, for the detection of this poison, a test which I pointed out to him, and which, from a variety of experiments, which we tried together, with a view to ascertain its comparative merits, we were induced to consider as the most effectual of all the tests hitherto used for that purpose. The method consists simply in adding, in succession, to the fluid suspected to contain arsenic, minute quantities of solutions of ammonia and of nitrate of silver; by which means, if the smallest quantity of arsenic be present, a dense yellow precipitate will be produced.

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* Read to the Medical and Chirurgical Society of London, in December last, and by them published. It is here inserted, not only on account of its intrinsic value, but because it bears reference to Mr. Sylvester's paper in our thirty-third volume.—N.

† I take this opportunity of stating, at Dr. Roget's request, that the patient, whose case he there related, completely recovered her health, and has remained well ever since.

Yellow precip. if arsenic be present.

All the particulars respecting this mode of detection having been fully stated by Dr. Roget, with such references to former writers on the subject as the case required, it would be quite superfluous to enter into any further detail on this head. My object in resuming the subject, the practical importance of which need not be pointed out, is to communicate to the Society the result of an inquiry which I have made on the nature of the yellow precipitate, the appearance of which is assumed as denoting the presence of arsenic, and to answer some objections which have been made against this test by Mr. Sylvester, of Derby, in a paper on metallic poisons, recently published in Nicholson's Journal*.

Objections by Mr. Sylvester.

The yellow compound in question has the following properties:

Properties of the yellow precipitate.

If, after being well washed with distilled water, it be suffered to stand for some time in an open vessel, it gradually passes to a brown colour; but it does not, like nitrate of silver, become black on continuing this exposure.

It is readily soluble in dilute nitric acid. It also dissolves on adding an excess of ammonia at the moment of its formation; but after it has been separated and dried, it is no longer sensibly soluble in ammonia.

If a small quantity of this precipitate be exposed to the heat of a lamp on a slip of laminated platina, a white smoke arises from it, and metallic silver remains attached to the platina. The reduction of the silver, in the form of a globule, is still more distinct and striking, if a little carbonaceous matter be mixed with the precipitate, and the blowpipe applied.

When the yellow precipitate, inclosed in a tube, is exposed to the heat of a lamp, the white smoke condenses on the cold part of the tube, in minute octoedral crystals of arsenious acid.

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* Nicholson's Journal for December, 1812. Vol. xxxiii. p. 306.

It is an arsenite of silver.

It appears, therefore, that the precipitate in question is a combination of white arsenic (arsenious acid) and silver, or an arsenite of silver; and it is inferred that its formation, when ammonia and nitrate of silver are added to a mixture containing arsenious acid, is owing to a double elective decomposition of the arsenite of ammonia, by the nitrate of silver, in consequence of which arsenite of silver is formed, and separates as an insoluble precipitate from the nitrate of ammonia which remains in the solution. The addition of ammonia is necessary, because arsenic acid alone cannot decompose nitrate of silver; but in Fowler's solution, in which the arsenic is already combined with an alkali, the decomposition takes place at once, without any addition of ammonia. The fixed alkalies, therefore, can answer a similar purpose; but ammonia has this advantage, that it does not, when added singly, decompose nitrate of silver, a circumstance which, in using the fixed alkalies, might occasion some confusion*.

Use and advantage of the ammonia.

Mr. Sylvester's objection; that muriatic acid would, if present, seize the silver.

With regard to Mr. Sylvester's objection, I shall, previous to my offering any remarks upon it, state it in his own words. "If ever muriatic acid be present," says this gentleman, "the test is then wholly useless, as a muriate of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing."

This danger of ambiguity, however, though applying in some degree to the process in question, and well deserving to be noticed, will be found to have been greatly overrated; and there are such easy and obvious means by which this ambiguity can be entirely removed, that it can make no solid objection to the utility of the test.

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* It is necessary, as Dr. Roget has observed in the paper already quoted, that the quantity of ammonia should not be too large; for in that case the precipitate is redissolved. But, even then, it may be made to reappear, by the addition of nitric acid in sufficient quantity to saturate the alkali. In this case, however, the precipitate is not permanent, owing, I find, to its being soluble in the nitrate of ammonia which is formed in the process. Carbonate of ammonia has also the property of producing and redissolving the precipitate.

The fixed alkalies in excess have not the power of redissolving the precipitate.

There cannot be the least doubt, as Mr. S. observes, but that whenever nitrate of silver is added to a solution containing muriatic acid, a precipitate of muriate of silver must be the consequence. But if the nitrate of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts.

Remedy. To add an excess of nitrate of silver; which will throw down the arsenic,

If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriate of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner, and becomes even more characteristic from a comparison with the white precipitate, the appearance of which differs from this in every respect.

and the arsenite of silver may be taken up by dil. nitric acid, and then precip. yellow by ammonia,

By this method, I believe that every objection to the test will be removed; and in order to anticipate all ambiguity, and to avoid any complication or practical difficulty in its application, I would propose to modify the process in the following manner:

To the suspected fluid, previously filtered, add, first, a little dilute nitric acid, and, afterwards, nitrate of silver, till it shall cease to produce any precipitate. The muriatic acid being thus removed, whilst the arsenious acid (if any, and in whatever state,) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain.

Easy manipulation, for this last process.

IV.

METEOROLOGICAL JOURNAL.

1812.	Wind.	BAROMETER. Max.	Min.	Med.	THERMOMETER. Max.	Min.	Med.	Evap.	Rain	
12th Mo										
DEC. 25	N	30 46	30·40	30 430	35	31	33 0			☾
26	N	30 50	30·40	30 450	37	30	33·5			
27	N	30·52	30 48	30 500	30	29	32·5		1	
28	W	30 52	30·32	30·420	43	32	37·5			
29	W	30 32	30·15	30 235	46	42	44·0			
30	W	30·15	29·92	30 035	50	42	46·0			
31	W	29 81	29·75	29·780	44	40	42 0			
1813.										
1st Mo.										
JAN. 1	W	30·09	29·81	29 950	45	38	41·5			
2	S W	30·26	30·09	30 175	44	36	40 0	6		●
3	W	30 30	30 20	30 280	41	34	37·5			
4	S E	30 30	30·09	30·195	42	34	38·0			
5	S W	30 09	29·80	29 975	44	37	40·5		5	
6	S W	29·77	29 70	29·735	50	40	45·0		9	☽
7	N W	29·70	29 30	29 500	46	40	43·0			
8	N W	29·02	29·30	29 460	48	28	38·0			
9	N W	29 87	29·75	29·810	41	31	36 0	9	9	
10	N W	29·82	29·70	29·700	34	28	31·0			
11	S E	29·80	29 70	29 750	40	26	33·0			
12	S E	29·70	29 61	29,055	34	29	31·5			
13	S E	29 58	29 53	29·555	38	34	36 0	5	0·16	○
14	N E	29·74	29·53	29·635	38	33	35·5			
15	N W	30 00	29 74	29 870	38	28	33 0			
16	E	30·20	30·00	30·100	44	29	36 5			
17	S E	30 20	30 04	30·120	35	28	31 5			
18	S E	30·14	30·04	30 090	31	30	30·5	-		
19	E	30 26	30·14	30·200	33	31	32·0			
20	N E	30·27	30 26	30·265	34	30	32 0			
21	N E	30 35	30 27	30·310	34	29	31·5			
22	N W	30 50	30 35	30·425	36	23	29·5	15		
		30·52	29·30	30 022	50	23	36 25	0·25	0·51	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

1812. *Twelfth Month.* 25. A very slight fall of snow. 27. A little snow last night. 30. 31. Windy night: small rain at intervals.

1813. *First Month.* 1. Small rain at intervals. 3. Misty morning. 5. Windy. 6. Windy: small rain. 7. Very misty, a. m. dark and cloudy, p. m. About 8, some lightning, which was soon followed by a shower. 9. Hoar frost: at 9, a. m. thick air, with *Cirrostratus* and *Cirrocumulus:* sounds come freely from the city, with the wind at S. S. W. Sleet and rain followed within an hour. 13. Overcast, a. m. thin sleet and rain. 14. Cloudy. 19. A little snow, a. m. 22. Clear, p. m. A fine red burst in the horizon at sun-set.

RESULTS.

Winds variable.

Barometer: greatest observed height, 30·52 in.; least 29·30 in.
Mean of the period 30·022 inches.

Thermometer: greatest height 50°; least 23°.
Mean of the period, 36·°25.

Evaporation 0·35 inches. Rain and snow 0·51 inches.

Plaistow,
First Month, 23, 1813.

L. HOWARD.

V.

On the Explosive Compound of Chlorine and Azote. By Messrs. R. PORRETT, *jun.* WM. WILSON, *and* RUPERT KIRK.

To Mr. Nicholson.

SIR,

Information received by the authors of the explosive compound of chlorine and azote.

IN the beginning of December, 1812, we learned from some of the newspapers, and from other sources, that a new explosive compound had been discovered at Cambridge, by Mr. Burton; that it was supposed to be a compound of chlorine and azote; that its explosive properties were of the most terrible kind, and had occasioned a very serious accident to Sir H. Davy, who was examining it; that the contact of oil would cause it to explode; that it was formed by exposing a solution of nitrate of ammonia to chlorine gas; and lastly, that the application of a freezing mixture during its formation was advantageous.

Such is the sum of the information which we then obtained, and which stimulated us to undertake a number of experiments with this compound; we have not since procured any further information respecting it, excepting such as we have derived from our own experiments. We state this, in order that your readers may have the means of distinguishing from among our experiments, those few which are not original.

Experiments.

We shall now proceed to relate our experiments, beginning with those which concern the formation of the compound.

Chlorine gas was received over warm water, in glasses of 16 cub. inches, and thence transferred to small basins of the ammon. solutions.

The mode which we adopted for forming it, was, in every instance, to fill with chlorine gas, over warm water, glass receivers of the capacity of about sixteen cubic inches; and to transfer these into small basins, containing the ammoniacal saline solutions. We soon found that the compound could be formed with solutions of other ammoniacal salts besides the nitrate: those which we have successfully employed for obtaining it, are the following:

Other ammon. salts which form the compound.

Sulphate of ammonia,
Phosphate do.
Muriate do.
Nitrate do.

Oxalate of ammonia,
Muriate of zinc, with excess of ammonia,
Muriate of ammonia, and iron by sublimation.

Those with which we did not succeed in forming it are the undermentioned.

Others which did not.

Carbonate of ammonia,
Triple muriate of platina and ammonia,
Sulphate of copper, with excess of ammonia,

We wished to ascertain whether any other solution containing azote might be substituted for the solution of ammoniacal salt. The solution which we tried with this view was one of nitrate of lead at a minimum, but we could not obtain by its means any of the explosive compound. We have not yet made any other experiments of this nature.

Nor did nitrate of lead, at the minimum.

There are certain bodies which, if present during the process for forming the explosive compound, prevent its formation, or at least prevent it from appearing. Of this description of bodies we have observed the following:

Sulphur. charcoal, carb. acid, atmos. air, and hidrog. gas prevent the compound being formed.

Sulphur, in solution in the ammonia,
Do. in powder within the receiver,
Charcoal in fine powder, adhering to the interior moist surface of the receiver,
Carbonic acid gas, equal in volume to one-third the chlorine gas,
Atmospheric air, do. do.
Hidrogen gas, equal in volume to the chlorine gas.

With respect to the temperature best adapted for the formation of the compound, our experiments lead us to quite an opposite conclusion from what has been published. The employment of a freezing mixture, instead of being advantageous, we have found to be the reverse, as we have never succeeded in obtaining the compound when the solution and the gas were at a temperature below 32°. In these instances, a thin crystalline icy film, was observed to line the sides of that part of the receiver containing the gas, and unless this was dissolved again by raising the temperature, no explosive compound was produced. On the contrary, when we have employed solutions of ammoniacal salts at the temperature of 90°, the explosive compound has been abundantly and quickly formed.

It is not formed at temperatures below the freezing point of water;

but best at higher temperatures.

Beautiful effect at 125 deg.

which ceased at 110 deg.

formed. In one experiment we heated the solution to 180°, and observed, in ten minutes after, when about half the gas was absorbed, and the temperature had lowered to 125°, that the receiver above the fluid was covered with the explosive compound, which trickled down to the surface of the solution in minute globules, which converged from all parts of the circumference of the circle forming the surface of the solution to the centre of that circle where they accumulated into larger globules. This phenomenon, which had a very beautiful appearance, seemed to us to be owing to a distillation of the compound from the central or hottest part, and a condensation at the exterior or coldest part of the receiver. This distillation ceased when the temperature had lowered to 110°, and the explosive compound then formed a film on the surface of the solution.

The phenomena attending the formation of the compound, are the following :

Phenomena of the formation of the compound.

Absorption of the gas, film of the compound seen after one-fourth of gas has been absorbed.

Globules of the compound which enlarge and sink.

As soon as the receiver of chlorine gas is placed in the solution of the ammoniacal salt, an absorption of the gas commences, and the solution rises slowly in the receiver. An action is apparent on the surface of the solution, which resembles small filaments reaching to the depth of about one-tenth of an inch. These filaments, on close inspection, appear to be composed of extremely minute bubbles of gas, ranged in a line one above another to the surface. When about one-fourth of the gas has disappeared, some of the explosive compound may generally be observed on the surface of the solution in a thin film ; the surface then looks oily, and appears divided, so as to give the idea of a map. As the solution rises in the receiver, the quantity of the explosive compound increases ; and it then collects into one or two flattened globules, which when they become very bulky, fall through the solution to the bottom. The whole of the gas is absorbed. The solution, after the formation of the compound, contains free muriatic acid, and also some of the compound in solution, if we may judge from its smell and yellow colour. We are not aware, that there are any other appearances during the formation of the compound, which are material to notice.

Theory.

The following appears, from our experiments, to be the theory of the formation of the explosive compound.

When an aqueous solution of muriate of ammonia is brought into contact with pure chlorine gas, one part of the chlorine is dissolved in the solution, and there decomposes the ammonia of the salt, by combining with its hydrogen, (with which it forms muriatic acid,) and sets free its azote, to combine with another part of the chlorine, with which it forms the explosive compound. The compound which is at first formed in this manner, is not visible because it is soluble in chlorine gas, and there is at first an excess of that present ; but in proportion as the quantity of this gas diminishes by combining with the elements of the ammonia, the explosive compound appears, and is deposited by the gas, generally on the surface of the solution, but sometimes considerably above it on the upper part of the receiver. The former effect is most likely to take place when the upper part of the receiver is in the form of a dome, or circular ; the latter, when it is in that of an inverted cone, or funnel shaped. The relative temperatures of the surface of the solution, and of that of the top of the receiver, have also, as might be expected, a considerable influence in determining where the compound shall be deposited. Its natural situation, from its high specific gravity, is at the bottom of the solution ; but unless it is in large quantity, or has been agitated, it remains where it is formed, on the surface of the fluid ; preserving that situation by taking a flattened spherical form, like that which a heavy oil assumes on the surface of water.

Part of the chlorine gas forms mur. acid with the hidrogen of the ammonia, and part forms the compound with its azote. The compound being soluble in the gas, is not visible till much of the gas is absorbed.

The explanation above given of the formation of the compound from solution of muriate of ammonia, applies equally when solutions of any other salt, formed of an incombustible acid and of ammonia, are employed ; the nature of the incombustible acid (with the exception of the carbonic) being of no importance, the only use of the acid being to prevent, by engaging the ammonia, the rapid action which the chlorine gas would exert on that alkali in an uncombined state : the existence of it in that state would also be incompatible with that of the explosive compound. This last assertion may appear extraordinary to those who know that the explosive compound may be formed by confining chlorine gas over a solution of pure ammonia ; but it is nevertheless true ; for in this case the explosive compound, although apparently formed from pure ammonia, is, in fact, formed from the muriate of that alkali ; which

This explanation applies to other ammoniacal salts.

which muriate is one of the products of the exposure of pure ammonia to chlorine gas.

The results differ with the proportions. If the free ammonia be in excess, muriate of ammonia only is formed, and azote set free.

Two different results are obtained from the mutual action of chlorine and ammonia, depending on the proportions of the two bodies presented to each other. Thus, when the quantity of ammonia present in a free state, is more than the chlorine gas can decompose and neutralize, the whole of the chlorine gas goes to the formation of muriate of ammonia, and no explosive compound is formed, but in its stead azotic gas is *found* at the termination of the experiment, equal in volume to one-third of that of the chlorine gas employed. Thus the only products of the experiment, under these circumstances, are the muriatic acid of the muriate of ammonia, and the azotic gas.

But when the chlorine gas is in excess, the last-mentioned azote is employed in forming the compound.

But when the quantity of chlorine gas present is more than is necessary to bring the ammonia to a neutral state; or, which is still better, when the ammonia has been previously neutralized by an acid, the azote, instead of remaining after the experiment in a state of gas, is found combined with the superabundant chlorine, forming the explosive compound. Thus the products of the experiment, conducted in this way, are, the muriatic acid which remains in the solution, and the explosive compound.

In the case first stated, the chlorine combines with one of the elements of the ammonia only, viz. the hidrogen; in that last described, it combines with both, viz. the hidrogen and the azote.

Experiment on the proportions.

We shall here relate an experiment made with the intention of ascertaining the proportions of chlorine and azotic gases, which, in a condensed state, form the explosive compound.

The compound was decomposed by potash and also by ammonia.

Two globules of the explosive compound produced from equal quantities of chlorine gas, and apparently of the same size, were decomposed; the one by potash dissolved in water, the other by solution of pure ammonia; the gases from each were collected and measured; that from the first was 0·8 of a cubic inch, and that from the last 1·1.

Phosphorus was heated in both; in that produced over the solution of potash it burnt, and caused its volume to diminish to 0·66; in that produced over the solution of ammonia, it did not burn, and caused its volume to increase to 1·3.

Now, if we suppose the two portions of gas, after the action of the phosphorus, to be in the same state, i. e. to be phosphuretted azotic gas, each containing, with respect to their volume, the same proportions of phosphorus, it will not be necessary, for the following calculation, to make any correction for the augmentation in bulk occasioned by the phosphorus; and as the circumstances of temperature and pressure were the same with both, neither will any corrections be necessary for those circumstances—we may, therefore, consider the comparative volumes of azotic gas produced in the two experiments, as represented by 66 and 130, and their difference as 64, being the excess of azotic gas produced over the ammoniacal solution. If we multiply this by 3, (the volume of chlorine gas necessary to produce 1 part of azotic gas from ammonia) we shall have 1·92, which will represent as gas the quantity of chlorine in one of the globules. And the quantity of azote, brought to the state of gas from the other, being, according to the first experiment, 0·66, makes the composition of the explosive compound to be nearly three parts of chlorine gas to one of azotic gas, condensed to a degree which we have not yet estimated.

Deduction that the new compound contained three parts chlorine gas and one azote.

We do not state this analysis as deserving much confidence—it must be *frequently* repeated before we can put any faith in it ourselves.

Our principal motive in describing the above experiment, before we have had an opportunity of repeating it, is to shew an easy and practicable mode of analysing the compound.

It may be proper now to describe some of the physical properties of the explosive compound.

Physical properties of the explosive compound.

Its colour is that of bees' wax; it is very fluid; it sinks, although with extreme slowness, in a solution of red sulphate of iron of the specific gravity of 1·578. Hence we conclude, that it must be of the specific gravity of about 1·6. It disappears after some time, even under the surface of water, or of the solution in which it was formed; but evaporates almost instantaneously when exposed to the air; it then diffuses its peculiar and penetrating odour through the surrounding atmosphere, which then affects the eyes in a very painful manner, causing them to shed tears. Its action on the lungs, however, we conceive to be much milder and less prejudicial than that of

Colour like bees' wax; very fluid; sp. grav. 1·6; soon disappears; very evaporable; smell offensive and noxious, but less so than of chlorine.

chlorine gas, as we have experienced very little inconvenience in this respect from standing close to a solution, from the surface of which the compound was diffusing itself into the atmosphere.

Very volatile, but may be kept in a close vessel.

The volatility of the compound is so great, as to present a considerable obstacle to preserving it; we have, however, found, that by limiting the quantity of air or of fluid which can come into contact with it, and at the same time preventing the escape of vapour by pressure, it can be kept for any length of time. We have accomplished this by introducing the compound into small tubes, closed at one end, about nine inches long, being first filled with some of the solution. The compound should occupy at least half an inch from the bottom of the tube, and some of the solution should afterwards be taken out to leave room for a little air, and to allow of the open end of the tube being hermetically sealed before the blowpipe. When any of these tubes are afterwards broken, the escape of compressed vapour is so considerable, as to occasion a loud report.

Difficult to transfer, because so volatile.

In our first experiments with the explosive compound, we experienced considerable difficulties in transferring it from one vessel to another, as we had no better mode than that of introducing into the solution and under the compound, a small spoon of tinned iron; the motion which this communicated to the compound often carried it to the surface, where it extended itself and disappeared, by dissolving in the atmosphere. In order to remedy this, and other inconveniences attending on this method, we invented a little instrument which we have found to answer our most sanguine expectations; it is formed of a small glass tube, of the size of a large writing quill, open at one end, and closed at the other, in the manner of a test tube, with the exception of a small circular hole in the centre. This tube is to be used as a syringe, the piston of which is to be formed of cotton, wound round a piston rod of wood or copper; by raising or depressing which, the explosive compound may be drawn into, or ejected from, the tube with the greatest facility. The peculiar advantages of this instrument are, its taking up the compound with so small a quantity of the solution, and with so much celerity, and its retaining it when the

A small instrument or glass syringe for taking up the compound.

tube is held in an inclined position, owing to the concave bed in which the compound lies; in short, this instrument combines the advantages of a spoon, with that of a common syringe. It is represented with a globule of the compound in it in the following outline—fig. 3, Plate V, where *a* is the glass tube, *b* the circular orifice, *c* the piston and rod, *d* a globule of the compound.

A precaution very necessary to be taken in the use of this instrument is, that it be clean, or at least free from oil, grease, or any combustible matter, which might, by causing the compound to explode, occasion a serious accident. This precaution is also very necessary with respect to all other vessels with which the compound may come into contact. Another general precaution, which we strongly recommend to those who may make experiments with this compound, is, to wear a mask on the face, and gloves on the hands. We conceive it also very proper to state, that although the results of upwards of two hundred different experiments which we have made with this compound are in favour of the conclusion, that it will not explode without the contact of a combustible body, or the application of a temperature exceeding 200°; yet three explosions have taken place, the causes of which remain unknown to us, as we were not aware of the compound being in contact with any other body than cold water. These explosions were, therefore, completely unexpected by us; but fortunately, they did not occasion any accidents of a serious nature.

Precautions against explosion.

Effects of different temperatures on the compound.

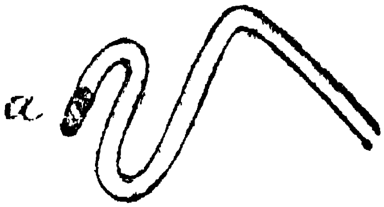
The effects of different temperatures on the compound we considered as very deserving of investigation, for which reason we made the following experiments:

It was not frozen at —16 deg.

A globule of the explosive compound was introduced into a small tube filled with water, it immediately fell to the bottom. The tube, with its contents, was then placed into a mixture of snow and nitric acid, into which a thermometer was also placed. The mercury fell to — 16°; the water in the tube was of course solidified, but the compound retained its fluidity, and was not altered in any respect.

At 160 deg. it came over by distillation.

A globule of the compound was introduced into a tube, closed at one end, of the form represented in fig. 4, pl. V: it was previously filled with the solution from which the compound was formed.



The globule is represented at *a*; the bent part of the tube was then placed in a vessel of water, and its open end immersed into a wider tube, also filled with some of the same solution. The vessel containing the water was then heated. When the temperature approached to 160°, it began to distil; at 160° the distillation was rapid, the compound being converted to vapour in the bent tube, which served as a retort, and was condensed and collected in that which was disposed as a receiver—much gas was given out during the process. The double curvature of the retort tube was to prevent any of the compound being floated over by bubbles of gas attaching themselves to it. This precaution we found was very necessary.

It did not explode at 200 deg.

A globule of the compound covered with water, contained in a little spoon of tinned iron, was with the spoon introduced into a quantity of water heated to 200°—this temperature was not sufficient to make it explode; it merely occasioned its vaporization.

But it did violently at 212

The last experiment was repeated, varying only the temperature of the water, which, in this instance, was 212°; the compound immediately exploded violently.

These experiments prove, that the explosive compound does not assume the solid form at — 16°, that it may be distilled at or below 160°, and does not explode but at a temperature above 200°, suddenly applied.

Our next object was to ascertain whether, when the natural pressure of the atmosphere was taken off, or diminished, from the explosive compound, it would still retain the fluid form, or whether it would assume the elastic state—with this view we made the following experiment:

Apparatus for exposin

A tube, 3½ inches long, closed at the bottom, had another tube of smaller bore, but of the same length, and open at both ends, placed within it; both tubes were then filled with mercury, excepting about one-fourth of an inch at the top. This one-fourth of an inch was afterwards filled with the following—

the compound in a vacuum.

1st. A small glass cup containing the explosive compound covered with a drop of muriate of lime. This cup moved freely within the tube.

2d. Muriate of lime in solution, surrounding and rising above the glass cup.

3d. A glass stopper, ground to the tube, and closing it accurately.

The compound, when in vacuo, assumed the state of vapour.

The inner tube was then raised thirty inches above the level of the mercury in the outer one. The column of mercury in the inner tube descended seven inches, leaving a column of twenty-three inches only. These seven inches were occupied by the explosive compound in a state of vapour: but as a little of it still remained in the cup, not converted to that state, a temperature of about 100° was applied to it—this caused it to disappear, and the vapour, after cooling, then occupied another inch, the mercurial column being reduced to twenty-two inches. The tube being now lowered until the mercury within and without it were of the same level, the explosive compound reappeared. There remained, however, seven-eighths of an inch of permanent gas, which an accident prevented us from examining; but we are inclined to believe, that this small quantity was produced when, on lowering the tube, the mercury rose into that part which had been occupied by the vapour, and the sides of which had been wetted with the liquid muriate of lime, which, notwithstanding that it was very concentrated, had probably absorbed some of the vapour, as we observed some bubbles of gas rising through the mercury from that portion of the metal which was in contact with the humid surface.

Experiment of exploding the vapour of the compound.

We repeated the above experiment in the hope, that by applying heat sufficient to make the vapour explode, we might, by this means, analyse the compound. We, therefore, exploded the vapour by surrounding the glass tube with part of a gun barrel heated nearly to redness; but at the instant of the explosion the tube was shattered. We, however, propose to repeat this experiment with a tube of greater strength

We have made a great many more experiments with the explosive compound; but as this communication is already of considerable length, we shall reserve the account of them for the next number of your Journal.

We are, Sir,
Your most obedient, humble Servants,
R. PORRET, Jun.
W. WILSON.
RUPERT KIRK.

London, 16*th Feb.* 1813.

VI.

A Statical Blow Pipe, with Remarks by C. L.

Instruments having fluid packing.

THE mercurial pump of Haskins, described by Desaguliers in his lectures; the water bellows of Hua blower in your 1st vol: octavo; the statical lamp, Edelcrantz, in your 5th volume, together with the gauge to Woulfe's most ingenious apparatus for heating water by waste steam, in your 2nd vol. are among the useful applications of a fluid substituted instead of packing, or leathering, for a moveable piece of the nature of a piston. On the present occasion, I send you an application of the same description to a blow pipe which acts upon the principle of the regulating piston in large works.

Description of the blow pipe.

The body of the instrument, BB EE, consists of a cylinder, having another interior cylinder, of rather smaller diameter, securely joined to the outer one, at the lower rim of the former, so that both cylinders are concentric, and both open at top; the edge of the inner cylinder being rather the highest. The outer cylinder is set air-tight in the foot E E, and communicates with the lower space D, which has connection with the mouth tube C, and the blow pipe D, by channels which have no other issue. A is a metallic cylinder, closed at the end A, and open at the other, which is the lower end. The letter C denotes a weight connected with the top of A by an inflexible wire or stem. This weight may be changed for one either greater or less, according to the intended force of the blast which is governed by it. The diameter of the cylinder A is such, that it may be inserted mouth downwards in the space between the other two cylinders; and if mercury be then poured into that space to about half its depth, the internal part of the apparatus will have no communication with the external air, except through G or F, and the mercury will stand on a level on both sides of A. But if the mouth be applied at G, and air blown in, it will be emitted again at F, with a velocity which will be greater the greater the pressure; but it will not be in the power of the operator to carry this pressure beyond a precise and steady limit. For the first effect will be to depress the mercury within the tube A, and at the same time to elevate the outer column or ring; and as soon as the difference between the heights of the internal and external mercury shall have become equal to that of a column of mercury, having the same base as that of the internal part of A, any farther effort will only cause A to ascend; and as soon as this ascent shall have carried the lower rim nearly to the height of the internal mercury, the air will make its escape through the mercury, by means of a notch made in the rim to determine the place of escape.

Chemists will perceive, that though this instrument possesses facility and precision of action, and is rendered a snug portable apparatus by a cylindrical cap which covers the whole by screwing on at EE; yet in point of invention it cannot claim to differ much from the modern gas holders. You have proved to us, on a former occasion, (Journal, quarto series, II. 35.) that the difference of 4-tenths of an inch of mercury was as much as blowing by the mouth can support or maintain; and this blast is sufficient for all the purposes of mineralogy and glass blowing. It may also be noted, that a reaction blow pipe, having a packed piston to re-act in a cylinder, and, I believe, another working piston with a valve and proper fittings in the same cylinder, was made many years ago by the celebrated Ramsden. But his compounded apparatus differs in many respects from the subject of the present description.

I am, Sir,
Your obliged Reader,
C. L.

VII.

VII.

Description of a simple Apparatus for Distillation. By a Correspondent

To Mr. Nicholson.

SIR,

THE number of ingenious and beautiful aparatuses for distillation, and the experiments of pneumatic chemistry, give a splendour to the exhibitions of lectures, and are highly gratifying to the affluent cultivators of science. But the greater part of operative chemists every day feel the expense which, from its own brittle nature, and the heavy duties imposed upon it, attends the use of glass. To them the simplicity and cheapness of a set of vessels stand among its most desirable properties. I send you a sketch of a combination which has not, I believe, the recommendation of novelty, but which, from repeated and habitual use, I have found of such value as leads me to believe you will be disposed to bring it into more public notice. It consists simply of A, a retort fitted into B at the neck E, which may be considered as the only indispensable ground joint. Into the other neck of B the vessel C is fitted like the upper vessel of Nooth's apparatus, having its neck D closed by a conical stopper, or, if preferred, a tube of safety may be placed there. In the operation, the distilled matter, or gas, passes over, and is received, condensed, or absorbed, in B. If the pressure be considerable, part of the liquid in B will rise into C, the included air of which last vessel may raise the stopper D, and partly escape.

If there be reason, from the nature of the subject, to apprehend, that part of the contents of the retort may boil over; or if the first products of distillation be required not to pass into B, the common adopter may be used, as shown by the dotted lines F. G. H. I.

I am,

Sir,

Your Constant Reader,

A.

VIII.

VIII.

Upon certain ready Processes for Computation, supposed to have been invented by the American Boy exhibited in London.*

SIR,

I SHALL make no apology for troubling you on a subject, which, though generally esteemed dry and abstruse, has at present acquired, from particular circumstances, considerable interest. There is a boy in town, who is exhibited as a curiosity, from the facility with which he performs several difficult arithmetical operations. It is pretended that this is a gift, and that he has had no instructions to enable him to do this. Now, Sir, as there are easy methods of solving these questions, which are not, I believe, generally known, I shall simply state them to the public, that this matter may, if necessary, be further investigated; and that this boy may be reduced to what he really is—a very clever boy, but no prodigy.

Introductory statement respecting the boy exhibited in London.

In extracting the cube root where it consists of three figures, it is well known that the first figure of the root may be obtained by a simple inspection of the number of millions, and the last figure, by observing the final figure of the number whose root is proposed to be extracted; if then, the middle figure could be found, we should have the root. To find this, square the final figure of the root so previously obtained; multiply this square by 3, call A the last figure of this product. Now cube the last figure of the root, substract its penultimate digit from the penultimate digit of the number given, (adding ten to this last, if it be the smaller of the two) call the result B.

In extracting the cube root of three periods, the first period determines the first figure, and the last digit determines the last figure, and the middle figure is found by a simple process.

Then that number, which being multiplied into A, produces a number terminating with the figure B, is the middle figure of the root. An example or two will make it manifest: suppose 377,933,067 to be proposed; here 7 is the first figure, (as $7^3=343$, the nearest cube below 377) and 3 is the last figure; since the cube of 3 terminates with 7, the last figure of the number. Now to find the middle figure $3^2\times3=27$, $A=7$, and $3^3=27$, of which the penultimate figure is 5.

Example of the extraction

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* From the Morning Chronicle of Feb. 17, last. For some account of Zerah Colburn, See our present vol, page 5.

Now the penultimate figure of the number is 6....$6-2=4=B$.

And since 2 7 (or A)$=14$, the last figure of which is 4 or B. The middle figure of root is 2, and root is 723.

Ambiguity of the rule.

This rule, I should add, becomes ambiguous in all cases where the number proposed terminates with an even digit, or with a 5; thus, in 41,421,736 $A=8$ and $B=2$.

Now, as either $4\times 8=32$ or $9\times 8=72$, it follows that, according to the rule, either 4 or 9 might be the middle figure, and either 346 or 396 the root; but as $\overline{396}\,|^3=$ nearly $\overline{400}\,|^3$, or 64 millions, it appears on inspection of the number proposed, that 346 must be the true answer. No error would, therefore, be produced by this ambiguity. Indeed, the only cases of ambiguity which can deceive, are in numbers terminating with 5.

The rule for extracting the square root is not much different

The rule for the square root differs only in these particulars; to determine A, take the simple power of the last figure of the root, and instead of 3, multiply by 2. To determine B. subtract the penultimate figure of the square instead of the cube of the last figure of the root. In all other respects, the two rules exactly agree. In the case of square, there is, however, an ambiguity which does not exist in the cube. It happens, that the final figure of a square number gives two figures which may terminate the root; as for instance, $4^2=16$ and $6^2=36$. If, therefore, a square number terminate with 6, its root may terminate with either 4 or 6, and, therefore, more mistakes will occur in the application of the rule. I believe this coincides with the fact; since the boy makes many more errors in the extraction of the square, than in that of the cube root.

—— but the ambiguity is greater.

Reference to a publication of Rallier des Ourmes, published 45 years ago, and containing the principles of these methods.

The principles of these rules, and the rules themselves, or a very slight modification of them, have been known so long ago as the year 1768; in that year, M. Rallier des Ourmes published two memoirs on the subject. They are to be found in Pp. 485 and 550 of the fifth volume of "Sçavans Etrangers." They are entitled "*Methode Nouvelle, &c.* or *a New Method of dividing, when the dividend is a multipile of the divisor, and of extracting the roots of perfect powers.* See page 550. His method only takes the last figures into account. In the extraction of the higher powers, this is undoubtedly the easier way. The second is, "Methode facile, &c. or *an easy Method of discovering all the*

“ *prime numbers contained in an unlimited series of odd numbers* “ *in succession, and at the same time, the simple divisors of* “ *those which are not primes.* This latter memoir is probably the method pursued by the boy to find prime numbers, and to resolve numbers into their factors. Of the method of M. Rallier, he himself says, “ *In a word, we do not hesitate to* “ *assert from experiment, that by this method, in a single day,* “ *and in the way of amusement, computations may be effected,* “ *which by the old methods, would require months of severe* “ *labour.*” I will only now add this observation. As the above rules depend upon the two or three first, and the two last figures of any number, it follows that the change of the intermediate ones cannot affect the result. If it should have occured to any one, as it has to me, *to have altered any of these*, and yet to have obtained the true result ; it will, I think, not be unfair to conclude, that either of these very methods, or some similar to them in principle, are those adopted. Let me add, that I have no doubt, but that any clever boy would, in a week's time, learn to apply those given above with the utmost facility.

——which are considered to be the same as those practised by the boy, and of easy acquisition by any boy of talent.

I am, Your's, &c.

A. H. E.

[The following is from the same respectable daily Journal of the 18th.]

SIR,

I agree with your correspondent A. H. E. that the young American is a very clever boy, but no prodigy, as one visit to him has convinced me. The ambiguity of the cases A. H. E. mentions, in extracting the cube root, may be readily cleared by any one conversant in figures in a few seconds, by finding B in the common formula for the cube root, which is the cube of the binomial $A+B$; namely, A^3+3A^2B, &c. which is, no doubt, perfectly well known to A. H. E. though to some of your readers, who may be interested in this matter, it may not be so familiar. For such the following directions may be useful. The first fig. of the root being known by inspection, take its cube from the millions given, then the remainder being

Remedy for the ambiguity.

being divided by the first two digits (for they will be sufficient) of thrice the square of the said first figure, will immediately shew which of the ambiguous figures should be taken for the second figure of the root. Thus, if the proposed number be 465,484,375, here the first and last digits of the root are 7 and 5: A=5 and B=5; any odd number, therefore, multiplied by A will give B; but if the cube of 7=343 be taken from 465, and the remainder 122 be divided by 14 (the first two digits of $7^2 \times 3$) it will be instantly seen that 9 is too great, and 5 is manifestly too little; there only remains 7, therefore, for the second digit of the root. The same method will easily clear the ambiguity when the proposed cube ends with an even digit.

I am, &c.

O.

IX.

On the Appearance and Disappearance of the Aurora Borealis.

By M. Cotte*.

Supposed connection between the aurora borealis and magnetism.

WHETHER there be any relation or agreement between the progressive changes of magnetical variation in a given latitude, and the times at which the aurora borealis appears, or ceases to manifest itself, is a question entitled to discussion. It is proved by observation,

Facts.

1. That the declination of the magnetic needle is not constant; that in our latitudes it was easterly before the year one thousand six hundred and sixty-six; and since that time it has more or less slowly increased to the west.

2. That the phenomenon of the aurora borealis, of which the western part of the atmosphere is also the seat, is seen frequently during certain epochas, and very seldom during others.

Agitation of the needle.

3. That when this phenomenon appears, it sometimes has an influence on the magnetic needle, so as to produce an irregularity of motion, or unsteadiness in the variation of the needle. The same thing sometimes happens in stormy weather, or when much electricity predominates in the atmosphere.

It must be remarked, that this influence of the aurora borealis upon the magnetic needle, does not constantly attend that phenomenon; and that a very feeble aurora borealis has sometimes a more marked influence upon the magnetic needle, than a very brilliant aurora borealis. It likewise happens not unfrequently, that the latter produces no sensible effect upon the magnetic needle. Upon the preceding facts, besides the question first above stated, I would propose the following:

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* Journal de Physique, LXXIII.

Whether the seat of the aurora borealis, in our latitudes before the year sixteen hundred and sixty-six, when the magnetic variation was easterly, was likewise in the eastern part of the atmosphere. And whether the times, when the variation is stationary, concur with the times of the disappearance of the aurora borealis; and those in which the variation of the needle is most rapidly changed, concur with the times of the most frequent appearance of the aurora borealis.

The want of accurate observations, before sixteen hundred and sixty-six, in both respects, renders the second question insoluble.

Minutes of the distinct periods of appearance and disappearance of the aurora borealis.

With regard to the first and the third question, the following table affords an outline of the observations which have been made upon the progress of the western declination of the needle since the year sixteen hundred and sixty-six, and the greater or less frequency of the appearance of the aurora borealis, for periods of ten years each.

					°	′		Times.
From	1666	to	1680	increase	1	30	the aurora borealis	7
——	1680	to	1689	——	5	20	——	13
——	1689	to	1700	——	2	12	——	22
——	1700	to	1710	——	2	38	——	59
——	1710	to	1720	——	2	10	——	112
——	1720	to	1724	stationary			——	531
——	1724	to	1730	increase	1	25		
——	1730	to	1740	——	1	5	——	349
——	1740	to	1750	——	1	45	——	84
——	1750	to	1760	——	1	15	no observations	——
——	1760	to	1770	——	1	25		
——	1770	to	1780	——	0	50	the aurora borealis	402
——	1780	to	1790	——	1	1	——	69
——	1790	to	1800	——	0	26	disappearance nearly total.	
——	1800	to	1809	diminish	0	12		

After submitting these observations, I shall only remark, that the nearly total disappearance of the phenomenon of the aurora borealis, which has taken place from the year seventeen hundred and ninety, to the present time, agrees with the diminution of the westerly variation of the magnetic needle, which likewise commenced nearly at the same time.

The observations contained in this notice, may be considered as the commencement of a series in which those afforded by future observers, will, no doubt, be more accurate and extended than what our predecessors have left us.

X.

*Description of a portable Instrument for ascertaining the quantity of Grain by weight, called the Chondrometer**.

The chondrometer consists of a small measure for corn, &c.

IN plate IV. Fig. 2. *A B C* represents a lever or balance moveable on the fulcrum *B* and supported by the stand *G*. The bucket *F* which in the instrument before me has the capacity of 8¾ cubic inches, is to be filled with grain, and when taken off and the handle turned back, may have its contents regulated by striking over the surface with a cylindrical straight piece, of about one-seventh of the diameter of the measure. The arm *B C*, carries a division, by means of which the sliding weight *E* can be set to counterpoise the bucket, and its contents at any weight of the latter between twenty-five and seventy-five pounds.

Method of measuring,

It scarcely need be observed, that the quality or product of any kind of grain or flour will, under like circumstances, be better the heavier its weight, and that a portable instrument to ascertain this must afford more accuracy than examination by hand. In the use of the present instrument very little instruction is necessary. The measure is to be filled in the same careful manner as a real bushel, and struck even by the rule, and not by a flat thin edge, which last would carry off too much of the grain; and rough grain such as oats or barley, should be charged a little heavier, because the proportion of these grains torn up by the striking in so small a vessel exceeds what happens in those of larger capacity. The charged measure is then to be hung in its place, and the weight being slided to the proper situation for making a fair counterpoise will indicate on the scale the number of pounds in each bushel of eight gallons.

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* The instrument, from which the drawing in the plate was taken, was made by Messrs. Page and Ovenden, in the Strand.

and weighing.

The weights per bushel of the nine following specimens of grain, as stated by Messrs. Payne and Ovenden, are as under :

Weights per bushel of several descriptions of grain.

		lbs.		lbs.		lbs.
Wheat	from	55	to	63	per bushel mean weight,	59
Rye	—	50	—	56	———	53
Barley	—	45	—	49	———	47
Oats	—	35	—	42	———	38½
Pease	—	62	—	67	———	64½
Small beans	—	60	—	66	———	63
Dutch clover	—	65	—	71	———	68
Canary	—	54	—	56	———	55
Rape	—	47	—	50	———	48½

XI.

Further Experiments and Observations on the influence of the Brain on the generation of Animal Heat. By B. C. BRODIE, *F. R. S.*

IN the Croonian Lecture for the year 1810*, I give an account of some experiments, which led me to conclude that the production of animal heat is very much under the influence of the nervous system. Some circumstances, which I have since met with, illustrate this subject, and seem to confirm the truth of my former conclusions.

Former experiments.

In an animal, which is under the influence of a poison, that operates by disturbing the functions of the brain, in proportion as the sensibility becomes impaired, so is the power of generating heat impaired also.

Disturbance of the brain impairs the animal heat,

If an animal is apparently dead from a poison of this de-

which is not restored by respiration,

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* Philos. Trans. 1811, or Philos. Journal XXIX. 359.

scription, and the circulation of the blood is afterwards maintained by means of artificial respiration, the generation of heat is found to be as completely destroyed, as if the head had been actually removed.

until the brain recovers.

Under these circumstances, if the artificial respiration is kept up until the effects of the poison cease, as the animal recovers his sensibility, so does he also recover the power of generating heat; but it is not till the nervous energy is completely restored, that heat is produced in sufficient quantity to counteract the cold of the surrounding atmosphere*.

In such circumstances the chemical changes from artificial respiration were as usual.

In the experiments formerly detailed, as well as in those just mentioned, I observed that the blood underwent the usual alteration of colour in the two systems of capillary vessels, while carbonic acid was evolved from the lungs at each expiration; and hence I was lead to believe, that the respiratory function was performed nearly as under ordinary circumstances, and that the usual chemical changes were produced on the blood. It appeared, however, desirable to obtain some more accurate knowledge on this point, and I have, therefore, instituted a series of experiments, for the purpose of ascertaining the relative quantities of air consumed in breathing, by animals in a natural state, and by animals in which the brain has ceased to perform its office, and I now have the honour of communicating an account of these experiments to this society.

It has been shewn, by Messrs. Allen and Pepys, first,† that every cubic inch of carbonic acid requires exactly a cubic inch of oxygen gas for its formation; secondly,‡ that when respiration is performed by a warm-blooded animal in atmospheric air, the azote remains unaltered, and the carbonic acid exactly equals, volume for volume, the oxygen gas, which disappears.

The watery vapour which escapes in respiration, is not formed during the process,

There is, therefore, reason to believe, that the watery vapour which escapes with the air in expiration, is not formed from the union of hydrogen with oxygen in the lungs, but that it is exhaled from the mucous membrane of the mouth and pharynx, resembling the watery exhalation which takes place from the peritonæum, or any other internal surface when exposed; and this conclusion appears to be fully confirmed by the experiments of M. Magendie, lately communicated to the National Institute of Paris.

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* The poison employed in this experiment should be the essential oil of almonds, or some other, the effects of which speedily subside. If the woorara is employed, so long a time elapses before the poison ceases to exert its influence, that it becomes necessary that the experiment should be made in a high temperature, otherwise the great loss of heat which takes place, is sufficient to prevent recovery.

† Phil. Trans. 1807, Part II.

‡ Phil. Trans. 1808, Part II. Ibid. 1809, Part II.

whence the changes in the air from respiration, may be deduced from the carbonic acid given out.

These circumstances are of importance in the present communication, which they render more simple, as they show, that in order to ascertain the changes produced on the air in respiration, it is only necessary to find the quantity of carbonic acid given out from the lungs. This becomes an exact measure of the oxygen consumed, and the azote of the air and the watery vapour expired, need not be taken into the account.

For the purpose of examining the changes produced on the air, by animals breathing under the different circumstances abovementioned, I contrived the apparatus, which is represented in the annexed Plate. Plate VI. Fig. 1.

Description of the Apparatus.

Apparatus.

A. Is a wooden stand in which is a circular groove ¾ of an inch in depth, and the same in width.

B. Is a bell-glass, the rim of which is received in the circular groove of the wooden stand. In the upper part of the bell-glass is an opening, admitting a tube connected with the bladder C.

D. Is a bottle of elastic gum, having a brass stop-cock E connected with it.

F. Is a silver tube, of which one end is adapted to receive the tube of the stop-cock E, while the other extremity, making a right angle with the rest of the tube, passes through a hole in the wooden stand, and projects into the cavity of the bell-glass, where it makes a second turn, also at a right angle, and becomes of a smaller diameter. In the upright part of the tube is an opening G.

The tubes are made perfectly air-tight, where connected with each other, and with the rest of the apparatus, and the circular groove is filled with quicksilver.

The capacity of the bell-glass, allowance being made for the rim, which is received in the groove with the quicksilver, is found to be 502 cubic inches. The capacity of the gum-bottle is 52 cubic inches, and in the calculations after the experiments, two cubic inches have been allowed for the air contained in the different tubes, and for the small remains of air in the bladder after being nearly emptied by pressure.

Mode of using the Apparatus.

Use of the apparatus in which an animal was inclosed, and the air afterwards examined.

In order to ascertain the quantity of air consumed under ordinary circumstances, the animal was placed on the stand under the bell-glass, the bladder being emptied by pressure, and the gum-bottle being distended with atmospheric air. During the experiment, by pressing occasionally on the gum-bottle, the air was forced from it into the bell-glass. On removing the pressure, the gum-bottle became filled by its own elasticity with air from the bell-glass. Thus the air was kept in a state of agitation, and the dilatation of the bladder prevented the air being forced through the quicksilver under the edge of the bell-glass. At the end of the experiment, the gum-bottle was completely emptied by pressure, and allowed to be again filled with air from the bell-glass: this was repeated two or three times, and the air in the bottle was then preserved for examination. The proportion of carbonic acid being ascertained, and the capacities of the different parts of the apparatus, and the space occupied by the animal being known, the total quantity of carbonic acid formed, and consequently of oxygen consumed, was easily estimated.

Artificial respiration conducted after the functions of the brain were destroyed.

When the experiment was made on an animal in whom the functions of the brain were destroyed, and in whom, therefore, voluntary respiration had ceased, the narrow extremity of the tube was inserted into an artificial opening in the trachea, and the animal being placed under the bell-glass, the lungs were inflated at regular intervals, by means of pressure made on the gum-bottle. The tube being smaller than the trachea, the greater portion of the air in expiration escaped by the side of the tube into the general cavity of the bell-glass, while the gum-bottle filled itself by its own elasticity with air through the opening G. At the end of the experiment, a portion of air was preserved for examination, and the quantity of carbonic acid was estimated in the way already described.

The air examined by a solution of potash over mercury.

The animals employed in these experiments were of the same species, and nearly of the same size. Attention to these circumstances was judged necessary, that the results might be as conclusive as possible. The chemical examination of the air was made by agitating it in a graduated measure over quicksilver, with a watery solution of potash. My friend, Mr. Brande, gave me his assistance in this part of the present investigation, as he had done on many former occasions. It will be observed, that in estimating the proportion of carbonic acid, no allowance has been made for that contained in the atmospheric air; first, because the quantity is so small that the omission can occasion no material error; and secondly, because the object is to ascertain, not so much the absolute, as the relative, quantities of carbonic acid evolved by animals breathing under different circumstances.

The experiments which I shall first notice, were made on the respiration of animals in a natural state.

Experiment 1. A rabbit by respiration for thirty minutes consumed about $50\frac{3}{5}$ cubic inches of oxigen per hour.

Experiment 1. Thermometer 65°, barometer not noted.

A young rabbit was allowed to remain under the bell-glass during thirty minutes. The respired air at the end of this time was found to contain $\frac{1}{20}$ of carbonic acid.

It was ascertained, that the rabbit occupied the space of 50 cubic inches.

The capacity of the bell-glass = 502 cubic inches.

That of the gum bottle 52 cubic inches.

The air in the tubes and bladder = two cubic inches.

$$\text{Then } \frac{502 + 52 + 2 - 50}{20} = \frac{506}{20} = 25{\cdot}3.$$

The rabbit, therefore, in thirty minutes gave out 25·3 cubic inches of carbonic acid, and consumed the same quantity of oxigen gas, which is at the rate of 50·6 in an hour.

Exp. 2. Another at the rate of $56\frac{2}{5}$ cubic inches.

Exp. 2. Thermometer 65°, barometer 30·1 inches.

A somewhat smaller rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air contained $\frac{1}{18}$ of carbonic acid. The animal occupied the space of 48 cubic inches.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28{\cdot}22.$$

The carbonic acid evolved was, therefore, equal to 28·22 cubic inches in half an hour, which is at the rate of 56·44 cubic inches in an hour.

Exp. 3.
Another consumed the same quantity.

Exp. 3. Thermometer 64°, barometer 30·2 inches.

A young rabbit, occupying the space of 48 cubic inches, was allowed to remain under the bell-glass during the same period as in the two former instances. The respired air contained $\frac{1}{18}$ of carbonic acid.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28\cdot22.$$

The results of this were, therefore, precisely the same as those of the last experiment.

These experiments were made with great care. The animals did not appear to suffer any inconvenience from their confinement, and their temperature was unaltered.

The next order of experiments were made for the purpose of ascertaining the quantity of air consumed by animals, in which the circulation of the blood was kept up by means of artificial respiration, after the brain had ceased to perform its functions.

Exp. 4.
Two rabbits were killed by dividing the spinal marrow. The heat fell off but the effect of artificial respiration on the air was not materially different.

Exp 4. Thermometer 65°, barometer not noted.

Having procured two rabbits of the same size and colour, I divided the spinal marrow in the upper part of the neck of one of them. An opening was made in the trachea, and the lungs were inflated at first by means of a small pair of bellows. Two ligatures were passed round the neck, one in the upper, and the other in the lower part behind the trachea. The ligatures were drawn tight, including every thing but the trachea; and the nerves, vessels, and other soft parts between them were divided with a bistoury. Eight minutes after the division of the spinal marrow, the thermometer in the rectum had sunk to 97. The animal was placed under a bell-glass, and the lungs were inflated by pressing on the gum-bottle about fifty times in a minute. When this process had been continued for thirty minutes, a portion of air was preserved for examination. The heart was found acting regularly, but slowly, the thermometer in the rectum had fallen to 90°.

The second rabbit was killed by dividing the spinal marrow about the same time when the experiment was begun on the first rabbit. Being in the same temperature, the time was noted when the thermometer in the rectum had fallen to 97°, and it was placed under another bell-glass, that it might be as nearly as possible under the same circumstances with the first rabbit. At the end of 30 minutes, the thermometer in the rectum had fallen from 97 to 91*.

The air respired by the first rabbit contained $\frac{1}{25}$ of carbonic acid. The bulk of the rabbit was found = 50 cubic inches.

$$\frac{502 + 52 + 2 - 50}{25} = \frac{506}{25} = 20.24.$$

20·24 cubic inches of carbonic acid were, therefore, extricated in 30 minutes, which is at the rate of 40·48 cubic inches in an hour.

The carbonic acid given out in the same space of time was less than in the former experiments; but it is to be observed, first, that in consequence of the ligatures the extent of the circulation was diminished; secondly, that in this instance one of the ligatures accidentally slipped, and an ounce of blood was lost in the beginning of the experiment.

As it was desirable to avoid any circumstances, which might occasion a difference in the results, in the subsequent experiments I employed animals, which had been inoculated with the poison of woorara, or the essential oil of almonds; by which means, while the functions of the brain were completely destroyed, the extent of the circulation was undiminished, and all chance of accidental hæmorrhage was avoided.

Exp 5. Two other rabbits were killed with woorara. The consumption of oxigen, by artificial respiration, in one of them, was not much less than before; but in the

Exp. 5. Thermometer 65°, barometer 29·8 inches

Two rabbits were procured, each occupying the space of 45 cubic inches. They were both inoculated with the woorara poison.

The first rabbit was apparently dead in nine minutes after the application of the poison; but the heart continued to act. The lungs were inflated for about two minutes, by means of a pair of bellows, when the thermometer in the rectum was

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* In measuring the heat of the rectum in these experiments, care is necessary that the thermometer should always be introduced to exactly the same distance from the external parts, otherwise no positive conclusion can be drawn relative to the loss of heat, as the more internal parts retain their heat longer than the superficial.

observed

other, which was not made to respire, the cooling was rather less rapid.

observed to stand at 98°. The animal was placed under the bell-glass, and artificial respiration was produced by means of pressure on the gum-bottle, as in the last experiment. At the end of 30 minutes, a portion of air was preserved for examination. The thermometer in the rectum had fallen to 91°. the heart still acted with regularity and strength.

The second rabbit died in a few minutes after the inoculation. The time was noted when the thermometer in the rectum had fallen to 98°, and he was placed under a bell-glass. At the end of 30 minutes, the thermometer in the rectum had fallen to 92°.

The air respired by the first rabbit contained $\frac{1}{20}$ of carbonic acid.

$$\frac{502 + 52 + 2 - 45}{20} = \frac{511}{20} = 25{\cdot}55$$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 51·1 cubic inches in an hour.

Exp. 6. Repetition with the like effect.

Exp. 6. Thermometer 66°, barometer 30·1 inches. Two rabbits, each occupying the space of 48 cubic inches, were inoculated with woorara.

In one of them, when apparently dead, the circulation was kept up by means of artificial respiration. He was placed in the apparatus under the bell-glass, and the lungs were inflated from 50 to 60 times in a minute. At this time the thermometer in the rectum stood at 97°. At the end of 35 minutes, a portion of air was preserved for examination. The thermometer had now fallen to 90°. The heart was still acting regularly.

The second rabbit was allowed to lie dead. When the thermometer in the rectum had fallen to 97°, he was placed under another bell-glass. At the end of 35 minutes, the thermometer had fallen to 90°·5.

The air respired by the first rabbit contained $\frac{1}{16}$ of carbonic acid.

$$\frac{502 + 2 + 52 - 48}{16} = \frac{508}{16} = 31{\cdot}75$$ cubic inches of carbonic acid evolved in 35 minutes, which is at the rate of 54·43 cubic inches in an hour.

Exp. 7. A rab-

Exp. 7. Thermometer 60°, barometer 30·2 inches.

The experiment was repeated on a rabbit, which had been inoculated with the essential oil of almonds. When he was placed under the bell-glass, the thermometer in the rectum stood at 96°. In a few minutes he gave signs of sensibility, and made efforts to breathe; but as these efforts were at long intervals, the artificial respiration was continued. In half an hour he breathed spontaneously 40 times in a minute. The thermometer in the rectum had fallen to 90°.

bit killed with essential oil of almonds. Revived by artificial respiration, but the cooling was not counteracted, &c.

The air being examined, was found to contain $\frac{1}{18}$ of carbonic acid.

The rabbit occupied the space of 47 cubic inches.

$\frac{502 + 52 + 2 - 47}{18} = \frac{509}{18} = 28{\cdot}275$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 56·55 cubic inches in an hour.

The animal lay as if in a state of profound sleep. At the end of two hours and twenty minutes, from the time of the poison being applied, the thermometer in the rectum had fallen to 79°, and he was again apparently dead; but the heart still continued acting, though feebly, and its action was kept up for 30 minutes longer by means of artificial breathing, when the thermometer had fallen to 76. The carbonic acid evolved during these last 30 minutes, amounted to nearly 13 cubic inches.

From the precautions with which these experiments were made, I am induced to hope that there can be no material error in their results. They appear to warrant the conclusion, that in an animal in which the brain has ceased to exercise its functions, although respiration continues to be performed, and the circulation of the blood is kept up to the natural standard, although the usual changes in the sensible qualities of the blood take place in the two capillary systems, and the same quantity of carbonic acid is formed as under ordinary circumstances; no heat is generated, and (in consequence of the cold air thrown into the lungs) the animal cools more rapidly than one which is actually dead.

Conclusion. That tho' artificial respiration in an animal, of which the brain is without action, will keep up the circulation, and produce the usual changes in the blood, it does not produce heat.

It is a circumstance deserving of notice, that so large a quantity of air should be consumed by the blood passing through the lungs, when the functions of the brain, and those of the organs dependant on it, are suspended. Perhaps it is not

What may be the nature of respiration?

not unreasonable to suppose, that by pursuing this line of investigation we may be enabled to arrive at some more precise knowledge respecting the nature of respiration, and the purposes which it answers in the animal economy. It would however be foreign to the plan of the present communication to enter into any speculations on this subject, and I shall, therefore, only remark, that the influence of the nervous system does not appear to be necessary to the production of the chemical changes, which the blood undergoes in consequence of exposure to the air in the lungs*.

The

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* The conclusion is directly contrary to that deduced by M. Dupuytren, from a series of curious experiments, made with a view to ascertain the effects which follow the division of the nerves of the par vagum, and it is an object of some importance in the present investigation, to ascertain in what manner the apparently opposite facts, observed by M. Dupuytren and myself, are to be reconciled with each other.

It was observed by this physiologist, that in an animal, in which both the nerves of the par vagum are divided, the blood returned from the lungs has a darker colour than natural, and that the animals, on whom this operation is performed, die sooner or later with symptoms of asphyxia, notwithstanding the air continues to enter the lungs; and hence he concludes, that the changes which are produced on the blood in respiration are not the result of a mere chemical process; but are dependent on the nervous influence, and cease to take place when the communication between the lungs and the brain is destroyed.

M. Provencal, in prosecuting this inquiry, ascertained that the animals subjected to this experiment give out less carbonic acid than before.

M. Blainville observed, that the frequency of the inspirations is much diminished, and M. Dumas restored the scarlet colour of the arterial blood by artificially inflating the lungs, and from these and other circumstances, he has arrived at conclusions very different from those of M. Dupuytren.

My own observations exactly correspond with those of MM. Dumas and Blainville. After the nerves of the par vagum are divided, a less quantity of carbonic acid is evolved, the inspirations are much diminished in frequency, and the blood in the arteries of the general system assumes a darker hue; but its natural colour may be restored by artificially inflating the lungs, so as to furnish a greater supply of air to the blood circulating through them.

We may suppose, that, on the division of these nerves, the sensibility

of

The facts now, as well as those formerly adduced, go far towards proving, that the temperature of warm-blooded animals is considerably under the influence of the nervous system; but what is the nature of the connection between them? whether is the brain directly or indirectly necessary to the production of heat? these are questions to which no answers can be given, except such as are purely hypothetical. At present we must be content with the knowledge of the insulated fact: future observations may, perhaps, enable us to refer it to some more general principle.

The temperature of warm-blooded animals is considerably influenced by the nervous system;

We have evidence, that, when the brain ceases to exercise its functions, although those of the heart and lungs continue to be performed, the animal loses the power of generating heat. It would, however, be absurd to argue from this fact, that the chemical changes of the blood in the lungs are in no way necessary to the production of heat, since we know of no instance in which it continues to take place after respiration has ceased.

but the heat is, nevertheless, connected with respiration.

It must be owned, that this part of physiology still presents an ample field for investigation.

Of opinions sanctioned by the names of Black, Laplace, Lavoisier, and Crawford, it is proper to speak with caution and respect, but without trespassing on these feelings, I may be allowed to say, that it does not appear to me that any of the theories hitherto proposed afford a very satisfactory explanation of the source of animal heat.

Where so many and such various chemical processes are going on, as in the living body, are we justified in selecting any one of these for the purpose of explaining the production of heat?

Can any one chemical process be selected as the cause of the heat?

To the original theory of Dr. Black, there is this unanswerable objection, that the temperature of the lungs is not greater than that of the rest of the system. To this objection, the ingenious and beautiful theory of Dr. Crawford is not open; but still it is founded on the same basis with that of Dr.

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of the lungs is either extremely impaired, or altogether destroyed, so that the animal does not feel the same desire to draw in fresh air: in consequence his inspirations become less frequent than natural, and hence arise the phenomena produced by this operation.

Dr. Black, "the conversion of oxygen into carbonic acid in the lungs," and hence it appears to be difficult to reconcile either of them with the results of the experiments which have been related.

Some objections considered.

It may, perhaps, be urged, that as in these experiments the secretions had nearly, if not entirely ceased, it is probable that the other changes, which take place in the capillary vessels had ceased also, and that although the action of the air on the blood might have been the same as under ordinary circumstances, there might not have been the same alteration in the specific heat of this fluid, as it flowed from the arteries into the veins. But, on this supposition, if the theory of Dr. Crawford be admitted as correct, there must have been a gradual, but enormous accumulation of latent heat in the blood, which we cannot suppose to have taken place without its nature having been entirely altered. If the blood undergoes the usual change in the capillary system of the pulmonary, it is probable that it must undergo the usual change in the capillary system of the greater circulation also, since these changes are obviously dependent on and connected with each other. The blood in the aorta and pulmonary veins was not more florid, and that in the vena cava and pulmonary artery was not less dark-coloured than under ordinary circumstances. We may, moreover, remark, that the most copious secretions in the whole body are those of the insensible perspiration from the skin, and of the watery vapour from the mouth and fauces, and the effect of these must be to lower rather than to raise the animal temperature. Under other circumstances, also, the diminution of the secretions is not observed to be attended with a diminution of heat. On the contrary, in the hot fit of a fever, when the scanty dark-coloured urine, dry skin, and parched mouth, indicate that scarcely any secretions are taking place, the temperature of the body is raised above the natural standard, to which it falls when the constitution returns to its natural state, and the secretions are restored.

Dr. Crawford's experiments upon the specific heat of blood *out of the body* and changed may be questioned.

It has been observed, by a distinguished chemist, that "the experiments to determine the specific heat of the blood are of so very delicate a nature, that it is difficult to receive them with perfect confidence"[*]. The experiments of Dr. Crawford, for this purpose, were necessarily made on blood out of the body, and at rest. Now, when blood is taken from the vessels, it immediately undergoes a remarkable chemical change, separating into a solid and a fluid part. This separation is not complete for some time; but whoever takes the pains to make observations on the subject, can hardly doubt that it begins to take place immediately on the blood being drawn. Can experiments on the blood, under these circumstances, lead to any very satisfactory conclusions, respecting the specific heat of blood circulating in the vessels of the body? The diluting the blood with large quantities of water, as proposed by Dr. Crawford, does not altogether remove the objection, for this only retards, it does not prevent coagulation, and some time must, at any rate, elapse, while the blood is flowing and the quantity is being measured, during which the separation of its solid and fluid parts will have begun to take place.

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[*] Thomson's History of the Royal Society, p. 129.

General reflections.

More might be said on this subject; but I feel anxious to avoid, as much as possible, controversial discussion. It is my wish not to advance opinions, but simply to state some facts, which I have met with in the course of my physiological investigations. These facts, I am willing to hope, possess some value; and they may, perhaps, lead to the developement of other facts of much greater importance. Physiology is yet in its infant state. It embraces a great number and variety of phenomena, and of these it is very difficult to obtain an accurate and satisfactory knowledge; but it is not unreasonable to expect, that by the successive labours of individuals, and the faithful register of their observations, it may at last be enabled to assume the form of a more perfect science.

XII.

Abstract of a Memoir upon the Origin and Generation of the Electric Power, whether by means of Friction, or in the Pile of Volta. By J. P. Dessaignes*.

Introduction.

I HAVE the honour to offer to the class, some enquiries into the origin and generation of electricity by friction, as well as in the pile of Volta. This subject must naturally create a lively interest among the learned of Europe, who are occupied on the discovery of the celebrated Volta; and I trust that my zeal for the progress of science, will receive confirmation from my attention to this object. The extent of my researches will not allow of their being read within the time usually allotted for that purpose. I shall, therefore, place my memoir on the table, and confine myself to indicate its principal results.

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* Read before the French Institute, Sept. 23, 1811.

Order of experiments. Electricity by friction—by contact—and by voltaic contact.

My enquiries are divided into four sections; 1. I examine the electricity of idio-electric bodies by friction, in mercury or upon wool; 2. and of the same bodies by simple contact with mercury; 3. and of metals by friction; and 4, that electricity which is produced by the contact of heterogeneous metals, or the pile of volta.

Three applications to mercury: 1. quick immersion; 2. slow immersion; 3. emersion.

Every one knows that idio-electric bodies plunged in mercury, become electric; but it has been hitherto unknown that there are circumstances under which they come out of the mercury without any electric virtue. I shall show those circumstances; but it will be useful in the first place to distinguish three kinds of immersion in mercury. 1. A brisk immersion in the manner of a blow; 2. slow immersion; 3. emersion, which consists in plunging a body in mercury, leaving it there for a longer or shorter time, and afterwards withdrawing it from the fluid. In the two first actions, the entrance and the taking out of the body from the mercury, are successive, and without any interval of repose. I must also observe, that in order to remove all suspicion of humidity, I have always kept those bodies upon which I operated, in a bottle of caustic lime, out of which I took them for each experiment. With these precautions I obtained the following results:

Glass, sulphur, amber, and sealing wax, immersed in mercury, are either electrified or not, according to the temperatures of the bodies and the mercury.

I. In the most favourable times for electricity, glass, sulphur, amber, and sealing wax, at the temperature of 10°. c. are not electric in mercury at 10°. c. by any of the three immersions; neither are they electric in any of the lower temperatures from 10°. c. to —18°. c. provided they be constantly at the same temperature as the mercury. Amber begins to be electric by the blow, and even by immersion, at 10°. c.; sulphur and sealing wax at 15°. c.; and glass at 20. c. but none of them become so by slow immersion at any degree of temperature.

Care must be taken to leave them in the mercury long enough to produce a perfect equilibrium of temperature, and afterwards to draw them out very slowly. The same bodies continue to become electric by the blow, or by quick immersion in temperatures superior to the degree at which their power begins to be produced. It is remarked, nevertheless, that as these bodies and the mercury arrive at much more elevated temperatures, their electric power becomes weak, and at last disappears, even by brisk immersion, at 80°. c. and 100°. c.

General result.

We may, therefore, establish it as a principle, that these four bodies are not electrical in mercury, when they are of the same temperature with it, and care is at the same time taken that no mechanical pressure be exerted, except that of the proper weight of the fluid upon the immerged body; and that even in the case when the immersion is effected with a strong pressure, these two united actions do not produce any electrical effect where the temperatures are equal, and are above 10°. c. and under 80 . c.

Electricity produced by plunging cotton, paper, silk, or wool, in mercury. Here the effects are also governed by the temperature, but differ from the preceding.

II. The effects are not the same with regard to cotton, paper, silk, and wool, for I have found these very electrical by the three immersions, and at all the degrees of heat between 10°. c. and 80° c. They continue to be electrical even below 10°. c.; but by lowering the temperature more and more, cotton is found to have its power extinguished in mercury at 3°. c. after remaining in it ten minutes; paper is also extinguished at 1°. c. after fifteen minutes; silk at 0°. c. after an hour and a half; and lastly, wool between 5°. and 6°. c. after remaining near two hours.

These four bodies are also extinguished, like the preceding, at elevated temperatures, and it is even remarked with regard to cotton and wool, that their power disappears at 60°. c. This arises from the humid principle of these bodies, which becomes fluid at that degree of heat.

It is very observable, that these bodies should also be electric by emersion, when they are at the same temperature as the mercury at all the degrees of the thermometer, in which their power is not extinguished, whereas, the contrary takes place with regard to all the preceding bodies. Their calorific fluid has, therefore, more natural tension than that of the four first bodies, and does not require an elevation of temperature to urge that of the mercury.

Effects of the third process, called emersion from mercury, upon the first class of bodies.

III. Glass, sulphur, amber, and sealing wax, are constantly electrical, even by immersion, when their temperature is a little higher than that of the mercury; a single degree of difference between the temperature of the rubbing body, and that which is rubbed, is then sufficient to determine an electric state, and this power is more intense, the greater the interval between the two temperatures. Nevertheless, there are limits beyond which it disappears; for example, when a cylinder of glass at 100°. c. is plunged in mercury at 18°. c. the glass then comes forth without electricity, provided the sudden contraction produced by the cold do not crack it, but if it do, the glass becomes extremely electric.

Singular effect of hot glass plunged in cold mercury.

If it crack, it becomes electrified, but otherwise not.

This want of excitability in very hot glass, plunged in very cold mercury, when it does not crack, appears to me to be an effect of the contraction of the glass which prevents the caloric from radiating outward, and forces it to radiate or return into the interior of its substance. It is, no doubt, from this reason, that workmen in glass houses can touch with impunity, a mass of red hot iron in fusion when plunged in water.

Explanation of a point of thermometrical difference.

I have asserted, that a single degree of difference of temperature between the mercury and the rubbed body, is sufficient to determine the electric state; but this is not to be understood but with regard to temperatures remote from the two extremes at which the electric power is extinguished. Thus sealing wax at 8°. c. is weakly electrified in mercury at 0°. c. and strongly at 8°. c. and so likewise at 4° c. it is no longer electrified in mercury at 0°. c. but it continues to be so by the stroke in mercury at 18°. c. The same thing is observed in all the other bodies at some differences in their degrees in relation to their specific heat. Silk, for example, at 0°. c. is still electric in mercury at 15°. c.; it is even so when itself at 4°. c. in mercury at 15°. c. but at 5°. c. it no longer shews any electricity.

Experiments in which the mercury was colder than the immersed body. The effects were much less.

IV. After having determined the influence of heat upon the electric power when it radiates from the body rubbed into mercury, I was desirous of seeing whether the same effect would take place when the heat should pass from the mercury into the immersed body. A tube of glass, particularly when the temperature of the mercury was from 60°. to 80°. acquired no electricity. The same was observed with glass rods; these came out, nevertheless, electric, when the temperature of the mercury was no more than 40°. or 50°. c.; but this electricity is so weak and disproportionate to that which takes place when the body is hot, and the mercury cold, that I have always considered it with surprise. In order to conceive the cause of this difference, it will be sufficient that I should observe, that a stick of glass at 75°. c. requires only two minutes to cool down through 50°. c. in mercury, at 12°. c. whereas, when the same stick at 12°. c. is plunged in mercury at 75°. c. it only causes a loss in the mercury of 4°. in the same period.

—— owing to the variation of temperature in the plunged body being greater.

V. To shew the influence of heat upon the electric power still more strikingly, I plunged a thick cylinder of glass in mercury at 80°. c. It first became weakly electric, as I have remarked, and afterwards non-excitable when its temperature was the same as that of the mercury. But sometime afterwards, and when the whole apparatus had advanced in cooling, I found it extremely electrical, and the power afterwards gradually became weaker accordingly, as by the progress of cooling, the internal between the temperatures had become less. When the mercury and the glass were entirely cooled, the electricity was no longer observable. It is evident that the electricity was here produced by the inequality of cooling which took place between the two bodies from their inequality of conducting power for heat.

Similar experiments extended.

VI. I endeavoured to determine the nature of the electricity of all these bodies plunged in mercury. Canton had asserted, that glass comes out of mercury in the positive state. Van Marum and Leroy found it negative. Ingenhousz found it positive by a slow immersion, and negative by a brisk one. After an attentive examination, I found that when the barometer stands high, and the air is inclined to cold, then glass, amber, wax, paper, cotton, silk, and wool, are always negative, whether the immersion be slow or brisk; but that they are, on the contrary, all positive, when the barometer is low, and the air inclined to be warm. It is worthy of remark, that sulphur is constantly positive at the same time when the other bodies were the most strongly negative. I must also observe, that during the whole summer, I found all the bodies positive in impure

Nature or kind of the electricity produced. It depends on the action, and also on the weather.

impure mercury, or such as was alloyed with tin, and negative at the same time in pure mercury.

Other experiments on temperature.

I made several experiments to ascertain the influence of temperature upon positive or negative electricity; one of which I shall mention. On the 10th of July, the wind being N. E. and clear, temperature 21°. c.; at 11h. in the morning, my heated glass rod came out of mercury in the negative state. I raised the temperature of the mercury to 100°. my rod was then without electricity, while the mercury imparted heat to it; but as soon as the whole began to cool, the rod became strongly negative in all its immersed parts. Some time afterwards, and constantly leaving it in the mercury, I found it positive at its extremity, and negative in all the rest. It must be remarked, that the vessel containing the mercury was conical, and the lower part being thin, the cooling was more rapid here than elsewhere. When the mercury was no higher here than 34°. c. the rod came out without electricity, but it continued to be so in the mercury at 26°. c. and constantly negative. Having then heated the rod a little above the mercury, at 34°. I withdrew it positive from the mercury; but it was always negative in mercury at 26°.

It is, therefore, established, that at equality of temperature, the rod is not excitable in mercury; though it is positive when it is only a little warmer than that fluid; and negative, when there is a great interval between the temperatures.

I must remark, that these different degrees of temperature do not change the electric state where the air tends to cold, or the barometer is low; for in the first case the rod is always negative, and in the second always positive.

Friction upon wool.

VII. These influences of the temperature upon the electric power, are not peculiar to mercury; they are observable likewise, in the friction of the [illegible] upon wool. In fact, if they be cooled in mercury at 12°. their electric power disappears equally in friction. It is not excited in amber until the sixth double friction, in wax at the eighth, in glass at the ninth, and in sulphur at the tenth. In this circumstance, if, after having exacted the electric power of glass, it be left at repose for 30 seconds, it is remarked that it becomes again non-excitable, and it will require four double frictions to reanimate it. It continues afterwards to become electric at each friction, if the action be continued, but is again extinguished if allowed an interval of repose. But at length, after five minutes alternate frictions, and intervals of repose, it becomes permanently electric, however great the interval between the friction. It may be conceived that this effect arises from the glass being a bad conductor, and allowing the heat produced by the friction to pass with difficulty to the centre of its substance.

The excitability of electric power is extinguished by cold and also by heat.

Not only cold extinguishes the electric power, but an elevated temperature has the same effects. Heat is no less efficacious in changing the nature of electricity; and in order to ascertain this, I varied, or rather repeated, the beautiful experiment of two skeins of silk performed by Bergman, in which we see that each of two skeins perfectly alike, becomes, in its turn, negative, and the other positive, when by the friction to which they are subjected, more heat is given to one than the other, and, consequently, more tension to its electric fluid

Experiments of producing electricity by mere contact of mercury without immersion.

VIII. Though the effects of heat and cold appear to me to be well shewn by the immersion of idio-electric bodies in mercury; nevertheless, as this kind of electrization gives a mechanical pressure of mercury against the immersed body, I determined to put the influence of temperature in a clearer point of view, by attempting to obtain the electric state by the mere contact of these bodies with the mercury. The following were the results:

Amber, sulphur, and glass.

1. Amber, sulphur, and glass, put into contact with mercury, and without any pressure, do not become electric, while at the same temperature as the fluid. But they become so when heated by the hand, and the slightest difference of temperature is then sufficient for the purpose, particularly when the air is becoming cold, and the barometer stands high.

Cotton, paper, silk, and wool.

2. I have always found cotton, paper, silk, and wool electrical by contact, whatever attention I have bestowed to bring them to an equal temperature with the mercury, provided they be always kept closed in a bottle of caustic lime.

The el. is greater, the greater the interval of temperature.

3. The electricity produced by contact, is always stronger the greater the interval of temperature between the two bodies which touch; yet, if the bodies be heated above 75°.c. and they be applied upon mercury, they acquire no electricity, and do not resume their power until a little cooled.

4. The same is true with regard to the inferior temperature, and those below 0.

A blow will produce electricity when the contacts are at equal temperatures.

5. When bodies have the same temperature as mercury, and the contact produces no electricity, the electric power may be excited by means of a smart blow of the body under experiment upon the surface of the mercury. This blow is sometimes ineffectual when the barometer is low. Here we already see the effect of the barometic pressure upon the electiic fluid; but much more evident proofs will be hereafter shewn.

(*To be concluded in our next.*)

XIII.

Account of the Drainage of a piece of Morass Land, called the Tarn, in the Parish of Clapham, in Yorkshire. By MAJOR B. HESLEDEN. (Soc. Arts XXX.)

Plan of the ground.

THE plan fig. 2. pl. V, describes the direction in which the principal or main drain, as also the cross drains, were severally carried, and A represents a spring of water, B the main drain, CCC, &c. smaller drains, D a small piece of dry ground.

Method of

The land consists of about twenty-one acres, and from its being encompassed on all sides by rising ground, the water was observed to spring from the bottom of the hill; consequently, the first drain was taken along its base and boundary of the Tarn, so as to receive the water on its first approach; the others were taken in the same direction, some of which, near the outward side, were made at the distance from seven to ten yards; but near the centre, at a greater distance, (viz.) ten to fifteen yards from each other, according to the dryness of the land. The principal, as well as the cross drains, were finished in the best possible manner, the bottom of them being invariably flagged, or laid with flat stones, (except in a few instances that happened to be firm clay), previous to its being walled on both sides, or soughed, then covered with flat stone, and afterwards filled to the top with earth and sod, which would be above the stone from one foot to half a yard in thickness.

This mode of draining, the Major would recommend to be always adopted, provided a sufficient quantity of stone can be procured, even if the expense attending it should be somewhat great, since it evidently must ensure its durability, almost for ever,—when in the ordinary way of draining, without first flagging, or laying the bottom with stone, it will in no great space of time give way by undermining the walling, or soughing part, besides being more liable to fill or choke up with earth or sediment. It would also be advisable to adopt (as was done in this instance with good effect) the letting in lots, of a given number of roods each, the cutting, stone-digging, soughing, or walling, &c. to different workmen; as best calculated to ensure the well finishing and due performance of each work.

Expenses.

The draining being completed, covered, sodded, and levelled, and the same covered with about three thousand horse loads of lime, and after adopting the greatest economy in the expenditure of this undertaking, the whole proved nearly as follows:

	L.	*s.*	*d.*
The main drain, cutting and blowing up of the rock, and carrying through the hill of the extent and depth so as to gain a sufficient fall......	92	15	0
The cross and other drains in the Tarn..........	181	7	0
Covering the whole with lime.................	91	0	0
	*L*365	2	0

Value of the land recovered gives 18 or 20 per cent. profit.

The value of the land in its improved state, in grass, was calculated to be worth, for the first two or three years, from two pounds ten, to five pounds fifteen shillings per acre; but the proprietor has not the least doubt, from his experience in laying lime upon the surface of land of that description, and in the same neighbourhood, that from the end of the first three years, it will be worth afterwards, and for some time, (for grazing or fattening of cattle) from three pounds ten, to three pounds fifteen shillings per acre, and, consequently, will pay for the money so expended, at least eighteen or twenty per cent.

XIV.

Respecting the Action of coloured Rays upon a Mixture of oximuriatic Gas, and hidrogen Gas. By Mr. Seebeck*.

Oximur. gas and hidrogen explode by solar light;

A MIXTURE of oximuriatic gas and hydrogen gas being exposed to the solar light, was suddenly decomposed by Messrs. Gay Lussac and Thenard. (See Recherches Physico-Chimiques, tom. 2, p. 189.) I repeated this experiment with success by means of the gasses which I had collected over hot water. I afterwards introduced these gasses under a glass vessel of a reddish yellow colour, and in another of a deep blue, which I exposed to the solar rays. Under the blue glass the decomposition immediately took place, without, however, any appearance of explosion, and in a minute at most it was terminated, and the glass was filled with water for the most part.

but not under coloured glass: blue light decomposes them much sooner than red, and neither with explosion.

Under the red glass, on the contrary, the decomposition took place very slowly. After twenty minutes of exposure to strong solar light, very little water had risen in the glass. This mixture of gas from the red glass was then introduced in the white glass, and exposed to the rays of the sun. No explosion took place; but in a few minutes the glass became filled with water. These experiments were frequently repeated, and always with the same result.

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* Translated from Schweigger's Journal of Chemistry, II, 263, by Vogel; from whose article in the Annales de Chimie, LXXXII, 323, I have extracted it.—N.

SCIENTIFIC NEWS.

Geological Society.

AT a meeting of this Society, on January 1, 1813, (the president in the chair) the reading of Mr. Philips's paper "on the Veins of Cornwall" was concluded.

The metalliferous veins of the Herland and Drannack mines run E b N and W b S, and the cross courses run N b W and S b E. The rock or country which they traverse is Schist, in some places so hard as to require being blasted. The width of most of the metalliferous veins varies from two inches to six inches: whenever exceeding this latter measure, they have been found soon after to divide and pass away in mere strings. A contre or oblique vein traverses these mines in a direction W b N and E b S, varying in width from one to three feet. Near the surface it was found to abound in blende and iron pyrites, but lower down afforded large quantities of copper ore. Whenever it intersected the metalliferous veins, the place of junction formed one lode for about eight fathoms in length, and three or four in width. The contre was heaved by the cross-courses, and these latter, at the place of intersection, are found to be not only enlarged but impregnated with ore. The contents of the cross courses are clay, quartz, or a mixture of both. It was in one of these cross courses, at the place of its junction with one of these metalliferous veins, that the celebrated deposit of silver was found mingled with galena, with iron pyrites, with bismuth, cobalt, and wolfram; and these substances were also found in those parts of the vein adjacent to the cross course.

Huel Alfred is in immediate contact with the mines just mentioned, and is at present one of the richest and most profitable copper mines that Cornwall can boast of. The great deposit of ore is contained in a contre from nine to twenty-four feet wide, which is considered as the continuation of that in Herland mine. The contre traverses a regular east and west vein, and it is remarkable that the ore, abundant as it is, has hitherto been found only in one mass at the depth of 117 fathoms at the point of junction of the contre and of the vein, giving off a branch 110 fathoms in length, along the eastern part of the same vein.

Another singular circumstance in this mine is, that one of the cross courses is heaved and intersected by an E and W vein.

Since the beginning of 1801, there have been sold about 45,000 tons of copper ore, the produce of Huel Alfred, for the sum of about 350,000l. of which the profit, divided among the adventurers, has amounted to about 120,000l.

January 15th.

The president in the chair.

A paper by William Conybeare, Esq. M. G. S. "On the origin of a remarkable class of organic impressions occurring in nodules of flint," was read.

This paper, which is chiefly occupied by detailed explanations of the drawings by which it is accompanied, relates to a class of substances thus characterized by Mr. Parkinson, in the second volume of his work on organic remains.

"Small round compressed bodies not exceeding the eighth "of an inch in their longest diameter, and horizontally disposed "are connected by processes nearly of the fineness of a hair, "which pass from different parts of each of these bodies, and "are attached to the surrounding ones; the whole of these "bodies being thus held in connexion." p. 75.

Mr. Parkinson conjectures, that the formation of these bodies has been the work of some polype similar to those by which the common zoophytes have been constructed, and, therefore, classes them among fossil corals of unknown genera. He observes, however, at the same time, that his reason for this arrangement is only a very slight analogy, as the objects in question differ materially from every known zoophyte, recent or fossil.

Mr. Conybeare having been so fortunate as to obtain several specimens of this fossil in a much better state of preservation than usual, shews clearly that they occur between the bony plates of a large bivalve shell, the Ostreo-pinnite of Walch, and in a similar situation in fragments of a striated shell, one of the patellites of Da Costa, which more probably, however, belongs to the genus ostrea. Similar substances have also been observed on the surface of a cast of the echinus. The matter of which these bodies are composed is flint, and they are supposed by Mr. Conybeare to be casts of the cells of some minute parasitical insect inhabiting the substance of the shells of certain species of the testaceous molluscæ, and probably deriving hence its nutriment either in whole or in part.

The anniversary meeting of the Society for the Election of Officers, &c. was held on Friday, the 5th of February, when the following members were elected.

President

The Hon. Henry Grey Bennet, M. P. F. R. S.

Vice-Presidents.

Sir Abraham Hume, Bart. M. P. F. R. and L. S.
Robert Ferguson, Esq. F. R. S.
Sir Henry Englefield, Bart. F. R. and L. S.
John Mac Culloch, M. D. F. L. S.

Treasurers.

William Hasledine Pepys, Esq. F. R. S.
Samuel Woods, Esq.

Secretaries.

Leonard Horner, Esq.
Arthur Aikin Esq.

Foreign Secretary.

Samuel Solly, Esq. F. R. S.

Council.

The Council consists of the above officers of the society, and of twelve other ordinary members.

Alexander Apsley, Esq.
William Blake, Esq. F. R. S.
J. G. Children, Esq. F. R. and L. S.
Samuel Davis, Esq. F. R. S.
James Franck, M. D.
G. B. Greenough, Esq. F. R. and L. S.
Alexander Jaffray, Esq.
James Laird, M. D.

James Parkinson, Esq.
Smithson Tennant, Esq. F. R. S.
Henry Warburton, Esq. F. R. S.
William Hyde Wollaston, M. D. Sec. R. S.

Keeper of the Museum and Draughtsman.
Mr. Thomas Webster

February 19*th.*

The president in the chair.

John Bostock, M. D. and Thomas Stewart Trail, M. D. of Liverpool, were elected members of the society.

A paper by John Taylor, Esq M. G S on the economy of the mines of Cornwall and Devon, was read.

The subjects treated on in this paper are,

1. The nature of the agreements between the owner of the soil and the mine-adventurers.

2. The arrangements between the partners, or the mine-adventurers themselves, and the system of controul and management appointed by them.

3. The mode of employing and paying the miners and workmen in use among the agents of the principal concerns.

4. The purchase of materials for carrying on the undertaking.

5. The sale of the ores from the mine-adventurers to the smelting companies.

1. The regulations of the *stannary* laws refer only to mines of tin; hence the search after, and working lodes of copper, lead, and other metals, is left open to such conditions as the adventurers and the lord of the soil can mutually agree upon. In general, the lord grants a lease for twenty-one years, determinable, however, at any time on his part if the mine should not be effectually worked. In return he requires a certain proportion, varying according to circumstances from an eighth to a thirty-second part of the ore, to be delivered to him in a merchantable state, or its value in money. He stipulates for a power of inspecting the works at all times, and binds the adventurers to maintain and leave, at any determination of the grant, all the shafts, adits, and levels, perfect and in good condition as to timbering.

2. The adventurers divide the whole concern into sixty-four

shares, which they distribute among themselves, and those who are allowed to join them, in various proportions. At the end of every two or three months, a general meeting of the adventurers is summoned, a statement of the accounts is laid before them, and the profit or loss is distributed to each, according to the amount of his shares. The general detail of management is usually delegated to one person, under whom are subordinate managers, called captains, selected among the working miners for their skill and character.

3. The work of the mires, both on the surface and below ground, is almost universally contracted for by the piece, at a kind of public auction held at the end of every two months; an accurate survey and measurement of the whole being previously taken by the captains. The lowest bidder has the *set*, and, in order to execute it, he associates to himself from one to eleven men, women, or children, according to the nature of the work. An account is then opened between the principal captain and the contractor, in which this latter is credited with all the tools, candles, gunpowder, and subsistence-money required by himself and his gang during the term; at the end of which the tools, and articles not used. are returned, the account is balanced, and the gain or loss on the contract is declared to the persons interested.

4. If materials for the use of the mine are purchased from those holders of shares who deal in the articles wanted (as is not unusual) great vigilance is required in the other proprietors to check the natural temptations to charge exorbitant prices, or to encourage a wasteful consumption.

5. The smelting companies for copper have seldom any share in the mines. There are about fifteen copper companies, all of which have agents and assay offices in Cornwall, though the smelting itself is carried on at Swansey. A weekly meeting is advertized to be held at some place near the principal mines, where the ores on hand, allotted into suitable parcels, (the produce of one mine being kept separate from that of another) are offered for sale. Previous to the day of sale, the persons intending to purchase attend at the mines for the purpose of taking samples, which are immediately put into the hands of the assay-masters. The agents for the smelting companies being thus furnished with the requisite information,

attend at the meeting, and each hands up to the chairman a note or ticket, containing the price per ton which he is disposed to give; the chairman then reads aloud the various offers, and the highest bidder is declared the purchaser.

Models of all the most Interesting Parts of the High Peak of Derbyshire.

MR. ELIAS HALL, *Fossilist and Petrefaction-Worker*, of Castleton, near Tideswell, in Derbyshire, has announced, that since the *mineral survey* of the county of Derby was undertaken for the board of agriculture, by *Mr. John Farey*, sen. and particularly since the publication of the first volume of his "Report on Derbyshire" by the Board; Mr. H. has assiduously applied himself to an examination of all the mineral limestone district and its vicinity, to the northward of Winster and Hartington, and to the carving out and completing of a *model* of its curious and rugged surface, under the patronage of his Grace the present *Duke of Devonshire*, and the Right Hon. *Sir Joseph Banks*, Bart.

On exact casts from this model he has contrived (as on Mr. Farey's mineral Maps) to represent, by colours, the eleven lowest of his principal rocks and strata of this district; viz. 1, fourth limestone (*ochre yellow*);—2, third toadstone (*red*);—3, third limestone (*grey blue*);—4, second toadstone (*bright yellow*);—5, second limestone (*green*);—6, first toadstone (*very dark blue*);—7, first limestone (*grey white*);—8, limestone shale (*reddish brown*);—9, first or millstone grit (*yellow*);—10, first coal shale (*dark brown*);—and 11, second grit rock (*dark brown*).—These several colours being painted on a fillet, in their proper order, by the side of actual *specimens* taken from the eleven rocks and strata above mentioned, are arranged and fixed on the E. side of the model, interspersed with other specimens of the chert, black marble, shale-freestone, coals, entrochi, &c. which belong to such strata.

Besides which, the ranges of all the principal *mineral veins* are represented, on the model, by *blue* lines for *rake veins*; and broad *blue* lines with *yellow* dots on them, for *pipe veins*; and specimens of lead ore in each limestone rock are given.

The great limestone and the Bakewell *faults* (of Mr. Farey's Report, Vol. I pp. 280 and 290,) are represented by a broad *red* line with *black* dots on it.

The turnpike roads are represented by small elevated lines; and the towns and villages, by small round elevations.

Printed labels are affixed to the several towns, villages, roads, hills, valleys, strata, mineral veins, caverns, &c.

The superficial scale of this model is one inch and a quarter to a mile. The scale for heights and depths necessarily exceeds the other, in order to give every hill and valley as nearly as possible the appearance that it had on the spots, where the carving was the greater part of it executed.

The colours are painted in oil, so as to be permanent, and admit of the model being cleaned from dust, &c.; and the whole is inclosed in a strong deal box, 20 inches long (from N to S,) 19 inches wide (from E to W,) and three inches deep, with a lid which takes off when the model is in use, and on the under side of which this description may be pasted and preserved.

For more readily understanding the internal parts of the district represented in the model, it should be observed, that the limestone-stone shale (reddish *brown*) occupies all the borders of the model, (but sometimes with first grit and coal shale upon it,) except for about six inches near its bottom, or S. end, between Sheen-Hill and Hartle-Moor: that this scale has an easy dip, or declines gently on the W. N. and E. sides, in those several directions, or with an easy rise towards the limestone and toadstone districts, whose strata have a general and rather a rapid dip towards the E. The four limestone rocks, coloured grey *white*, *green*, grey *blue*, and ochre *yellow*, dip successively under the eastern shale, and each other, in this order: and the three toadstones, coloured dark *blue*, bright *yellow*, and *red*, dip also to the E. between the limestone rocks.

The coloured patches and rings will point out the several *hummocks* and *denudated patches* of strata, that are detached from the masses or surfaces of the seven strata of limestone and toadstone which are mentioned above.

Mr. Hall's own examination of the *strata* of the considerable district comprised in his model was separately conducted, and afterwards compared with Mr. Farey's report and manuscript map;

map ; and every spot, wherein any difference of the two surveys appeared, has been visited again and again ; all the old and best-informed miners have been consulted, and by repeated correspondence, and the liberal and ready communications of Mr. Farey, he considers himself as warranted to present his models, as faithful representations of the numerous and highly curious phenomena which the High Peak presents ; any of which he is ready to explain minutely on the spots, and, in other respects to assist the investigations of curious travellers who may be anxious to examine and verify these facts, and wish to engage his personal assistance for such purpose. It is also a part of Mr. Hall's professional business to make and label ample *specimens* of all the various *mineral productions of the Peak Hundreds* ; carefully noting their precise localities, and their places in the strata or veins (a species of information too rarely met with, even in the best mineral collections.) He always keeps a large collection of the *Derbyshire minerals*, for sale, collected almost entirely by himself

Mr. Farey, from a desire to promote mineral science, and to serve Mr. Hall, has consented to keep some of his models, at his house, No. 12, *Upper Crown-street, Westminster, London*, for inspection, and sale, at eight guineas each. They may also be had, on these terms, of Mr. Hall himself, as above, or by application to him by letter.

Mr. BAKEWELL will commence a course of geological lectures in March, at Willis's rooms King-street, St. James's, designed to illustrate the geology and mineralogy of England, and particularly intended to direct the attention of landed proprietors to the neglected mineral treasures on their own estates. Mr. Bakewell also intends shortly to publish, in 1 Vol. 8vo. a work entitled Outlines of Geology, with observations on the Geology of England.

Speaking Automaton or Machine.

Mr. ROBERTSON, whose name has frequently appeared in the Annales de Chimie, but is better known to the public as an aeronaut in Denmark, has, it is said, contrived a speaking figure, which he exhibited a few months ago at Paris. It articulates the words *Papa, Mamma,* and *Vive Napoleon,* and daily improves in power, from (as may be supposed) the practice of the person who works it.

On this occasion I would remind some of my readers, who may have remembered the automaton chess player, which was exhibited in St. James's Street, about thirty years ago by the Baron Kempellen,—that that mechanic shewed, in a kind of half private exhibition in his parlour, after the games at chess were over, an instrument which spoke. I was present at one of these performances The Baron said the machine was not then completed. It was a kind of box which he brought out and placed upon a table. Speaking without any memorandums at so considerable a distance of time, I judge its dimensions to have been about two feet in length, one foot wide, and eight or nine inches deep. It had no lid; but we were prevented from seeing the inside by a cloth which covered it. The Baron put his hands into the box under the cloth, so that his right arm was disposed longitudinally in the box, and seemed to press a pair of bellows: the other hand was put in, crosswise at the end, near the place of the right-hand, and seemed to be employed with keys or some apparatus, or perhaps both hands may have been so employed. When he caused the instrument to speak, he raised his right elbow and gradually pressing it down, the sound was heard. It was a clear monotonous sound as if from a single pipe about the pitch of D, above the middle C, concert pitch; and the words *papa* and *mamma* were uttered, very distinctly in a slow drawling manner; that is to say there was a want of the usual modulation of speaking tones, and the sound fell off in its intensity towards the end. After several other words had been spoken, a lady asked in French if it could not speak sentences, and the Baron asked what it should say: she answered *que se suis mechante,* and the instrument said *vous etes mechante, mais vous etes aussi bonne.*

Kratzenstein has given some account of the principles of an engine of this kind in a memoir extracted in the Journal de Physique, and Dr. Young has cursorily mentioned the subject in his lectures, with some diagrams.

Quantity of Spirits and other fluids ascertained by Weight.

MRS. LOVI has lately communicated a memoir to the Edinburgh Institute, upon the advantages in point of accuracy of measuring fluids by weight instead of using vessels of known magnitude. She is mentioned as patentee of the areometrical beads, which have been now upon sale in London for a considerable number of years, and consist of small glass balls hermetically sealed, having their specific gravities written upon them. The principal objection to these is their number and brittleness, and, perhaps, the difficulty of making them of so small a size, with as much accuracy as the larger ball, of the usual floating instrument.

With regard to the practice recommended of weighing fluids instead of measuring them, it is grounded on the considerations, 1. That weights are usually made with more exactness than measures. 2. The measuring multiplies error by a series of operations, 3. and it has been questioned whether a single measure of a fluid could be had to the same degree of precision as a mass determined by weight; though by the common figure given to the copper measures which terminate in a neck or throat having a small surface, the precision required in business may, no doubt, be had.—Considerations of temperature affect both methods alike, and I apprehend that, though the temperature of spirits is attended to, in determining their strength, yet it is neglected in taking the measures of quantity; which it is liable to affect as far as 2 per cent, or one gallon in 50, and ought, therefore, to be considered.

I have seen oil sold retail by the gallon weight in London, which is certainly very fair for the buyer, who might else in that adhesive fluid, lose as much as hangs to the vessel every time it is emptied.

New

New Publications.

The Gentleman's Mathematical Companion, for the Year 1813; containing answers to the last year's enigmas, &c. &c. Price 2s. 6d.

Elements of Universal Geography, ancient and modern; with historical, classical, and mythological notes. By A. Picquet. 12mo. 5s.

A Sketch of the Sikhs, a singular nation, who inhabit the provinces of the Penjab, situated between the rivers Jumna and Indus; by Sir W. Malcolm, 8vo. 8s. 6d.

Asiatic Researches; or, Transactions of the Society instituted in Bengal. Vol. XI. octavo 18s. or quarto two guineas.

Topographical Dictionary of Yorkshire. By W. Langdale. 8vo. 10s. 6d.

The Picture of London for 1813. 6s. 6d.

Travels in South America. 4to. 2l. 2s. forming vol. XIV of Pinkerton's General Collection of Voyages and Travels.

General Collection of Voyages and Travels. Part LVIII. 4to. 10s. 6d.

Journal of a Residence in India; by Major Graham. 4to. 1l. 11s. 6d. boards.

Rees's New Cyclopedia. Vol. XXIII. Part I. 1l. or large paper, 1l. 16s.

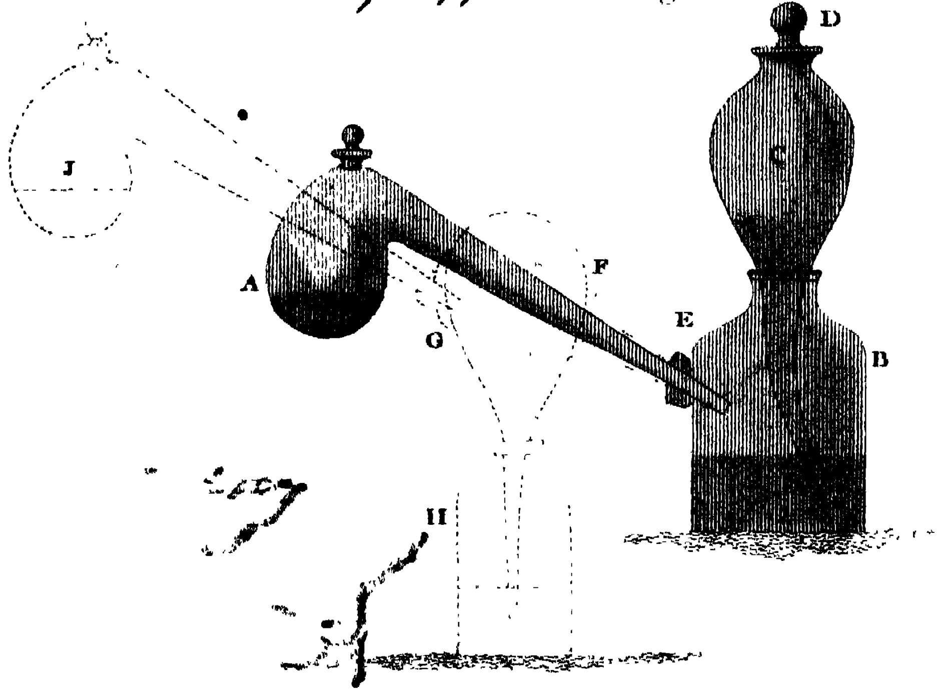
Correct Tide Tables for the Year 1813, shewing the true time of high water in the morning and afternoon of every day in the year, for all the principal ports and places in Europe and America. By William Adams. 1s.

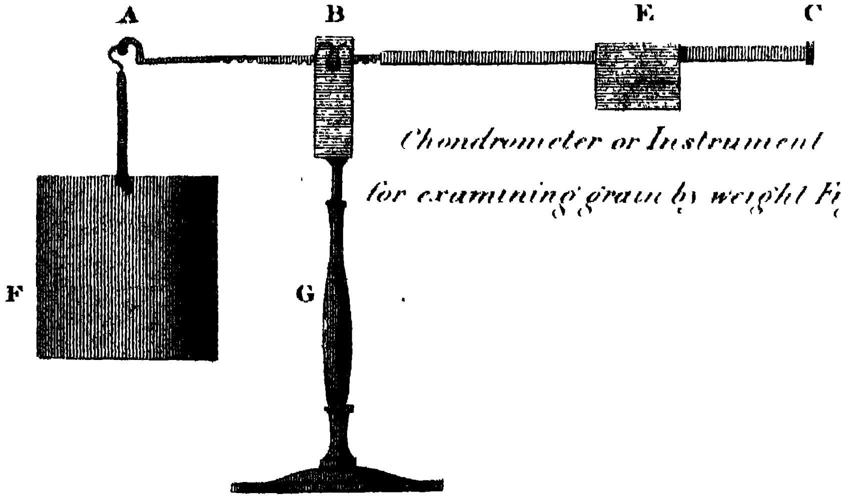
Sir H. Davy will soon publish Elements of Agricultural Chemistry, in a Course of Lectures delivered before the Board of Agriculture.

A new edition of Smeaton's Account of the Building of Edystone Light-house has been announced, and is considerably advanced.

Professor Playfair is printing the second part of his Outlines of Natural Philosophy; and also a new edition of his Illustrations of the Huttonian Theory.

Distillatory Apparatus Fig 1.





Chondrometer or Instrument for examining grain by weight Fig 2

Statical Blow pipe Fig 1

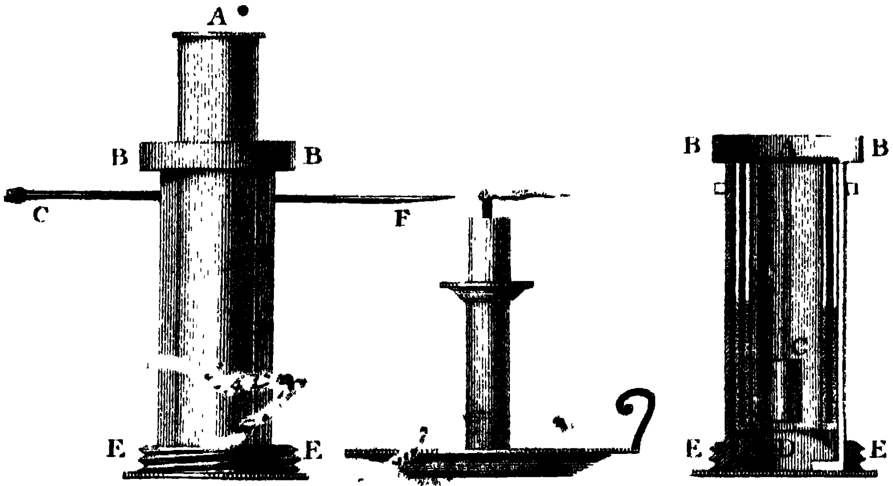


Fig. 4.

Fig: 3.

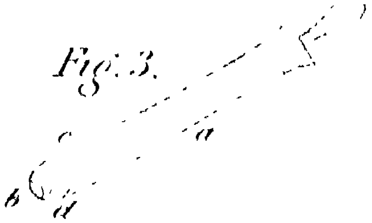
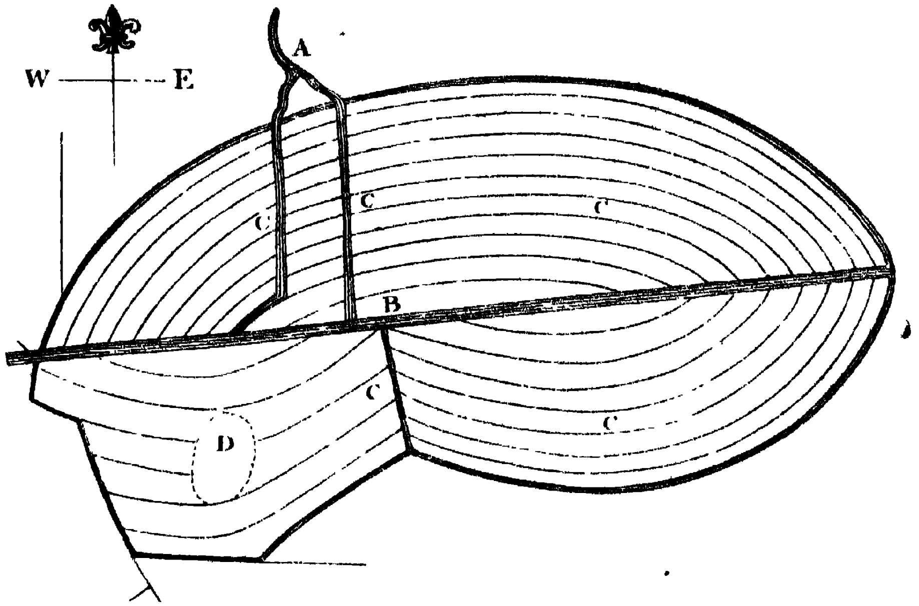


Fig 2



A

JOURNAL

OF

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AND

THE ARTS.

APRIL, 1813.

ARTICLE I.

Experiments on the comparative Strength of Men and Horses, applicable to the Movement of Machines. By M. Schulze*.

Importance of measuring the force of men and animals as first movers

THOSE who have had occasion to construct machines intended to be moved by men or animals, are sufficiently aware how important it is to be acquainted with the quantity of power that can be attributed to either of them, in order to estimate with accuracy the effect which it is proposed to obtain from the machine. It is well known, that the arrangement of the whole depends entirely on the ratio of the velocity of the motive force to the resistance. This was the reason that long ago induced experimentalists to take the trouble of determining the strength as well as the velocity exerted by men and animals, when they are made to move machinery; and the results they obtained, which have been commonly made use of in computing the effect of machines, are, that men exert from twenty-seven to thirty pounds, with a velocity of from one and a half to two feet per second; and that a horse has about seven times more strength than a man, with a velocity of from four to six feet per second.

Formulas of

These are the data which we have been obliged to use when-

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* Memoirs of the Royal Academy of Sciences of Berlin for 1783.

Euler for determining the effects of machines moved with different velocities, &c.

ever it became necessary to compute the effect of a machine moved by men or horses. It is evident that the force must be diminished when the velocity is increased, and *vice versâ*: but we are not yet certain of the method of finding the ratio of the diminution or augmentation of this force to the velocity. Euler has given us two different formulæ to compute this ratio: but no one has hitherto attempted to verify by experiment which of them is to be preferred, although they differ very considerably from each other. If we put P for the absolute force which takes place when we simply consider equilibrium, C the absolute velocity which takes place when the man or animal moves freely, and without being overcome by the resistance, p the relative force, and c the corresponding velocity, we have by the first of these formulæ,

$$p = P\left(1 - \frac{c}{C}\right)^2 \text{; whereas the second gives us } p = P\left(1 - \frac{c^2}{C^2}\right)$$

As I am obliged now more than ever to attend to a number of machines, and to compute their effect, it therefore concerns me very much to know exactly in what manner to estimate, compare, and fix the strength and velocity of men and animals, which are used for moving various machines, proper for different purposes.

With this view I made, with considerable care, the experiments I am now about to detail, which of course would have been very expensive, had I not had some facilities which other persons may not possess.

Experiments with men.

To make the experiments on human strength, I took promiscuously twenty men of different sizes and constitutions, whom I measured and weighed; the result of which is given in the following table:

Their sizes and weights.

Order.	Size.			Weight.	Order.	Size.			Weight.
1	5′	3″	4‴	122	11	5′	9″	7‴	132
2	5	2	3	134	12	5	1	4	157
3	5	7	2	165	13	5	3	2	175
4	5	5	0	131	14	5	4	1	117
5	5	11	2	177	15	5	10	8	192
6	6	0	4	158	16	5	0	3	133
7	5	8	3	180	17	4	11	2	147
8	5	2	1	117	18	5	3	9	124
9	5	4	8	140	19	5	6	0	163
10	5	0	4	126	20	5	10	1	181

To find the strength that each of these men might exert to raise a weight vertically, I made the following experiments :

Experiments upon their strengths

I took various weights, increasing by 10lbs. from 150lbs. up to 250lbs. All these weights were of lead, having circular and equal bases. To use them with success in the proposed experiments, I had at the same time a kind of bench made, in the middle of which was a hole of the same size as the base of my weights: this hole was shut by a circular cover, which effected this purpose when pressed against the bench, but at other times was kept at about the distance of a foot and a half above the bench, by means of a spring and some iron bars. To prevent the weight with which this cover was loaded during the experiment, from forcing down the cover lower than the level of the surface of the bench, I had several grooves made in the four iron bars, which sustained the cover at any height at which it might arrive by the pressure of the springs, as soon as the pressure of the weight ceased.

in raising a weight vertically.

After having laid the 150lbs. on the cover, and the other weights in succession, increasing by 10lbs. up to 250lbs. I made the following experiments with the men whose size and weight are given above, by making them lift up the weights as vertically as possible all at once, and by observing the height to which they were able to lift them. The following table gives the heights observed for the different weights marked at the head of the table.

	150	160			190	200	210	220	230	240
1	7″ 9‴	6″ 4″	4″ 11‴	4″ 4‴	3″ 8‴	2″ 8‴	1″ 1′			
2	7 10	6 6	5 7	4 7	3 11	2 5	0 5			
3	7 9	7 3	6 5	5 9	4 11	4 0	3 0	1″ 7‴	0″ 3‴	
4	8 3	7 6	7 2	5 10	5 3	4 7	4 0	3 8	3 1	1′ 4‴
5	12 4	11 1	9 7	8 5	7 10	7 1	5 10	4 7	3 2	1 3
6	14 5	14 0	13 5	12 8	11 5	10 1	8 6	6 6	4 1	0 1
7	12 11	11 3	10 5	9 3	8 1	6 9	5 3	3 8	1 11	0 2
8	11 9	10 2	9 4	8 11	8 1	6 11	5 10	5 1	3 2	1 0
9	9 5	8 3	7 1	5 6	4 1	2 9	1 3			
10	8 1	6 5	4 7	3 9	2 5	1 7	0 4			

Results.

This table proves to us, that the size of the men employed to raise the weights vertically, has considerable influence on the height to which they severally brought the same weight. We find also by this, that the height diminishes in a much more considerable ratio than the weight increases; and we may therefore conclude, that it is advantageous to employ large men when it becomes necessary to draw vertically from below upwards; and, on the contrary, it is more advantageous to employ men of considerable weight, when it is required to lift up loads by means of a pulley, about which a cord passes, which the workmen draw in a vertical direction, from above downwards. To find the absolute strength of these men in a horizontal direction, I took the following method:

Having fixed over an open pit a brass pulley, extremely well made, of fifteen inches diameter, whose axis, made of well-polished steel, to diminish the friction, was three-fourths of an inch in diameter; I passed over this pulley a silk cord worked with care, to give it both the necessary strength and flexibility. One of the ends of this cord carried a hook to hang a weight to it, which hung vertically in the pit, whilst the other end was held by one of the twenty men, who, in the first order of the following experiments, made it pass above his shoulders; instead of which, in the second, he simply held it by his hands.

I had taken the precaution to construct this in such a manner, that the pulley might be raised or lowered at pleasure, in order to keep the end of the cord held by the man always in a horizontal direction, according as the man was tall or short, and exerted his strength in any given direction.

I had made the necessary arrangements, so as to be able to load successively the basin of a balance which I had attached to the hook at the end of the cord which descended into the pit, whilst the man who held the other end of the cord employed all his strength without advancing or retracting a single inch.

Experiments with men pulling horizontally.

The following table gives the weights placed in the basin when the workmen were obliged to give up, having no longer sufficient strength to sustain the pressure occasioned by the weight. To proceed with certainty, I increased the weight each time by five pounds, beginning from 60, and intervals of time, having always precisely a space of ten seconds between them. The result of these observations, repeated several days in succession, is contained in the following table:

When the cord passed over the shoulders of the workmen :

Order.	lbs.	Order.	lbs.	Order.	lbs.	Order.	lbs.
1	95	6	100	11	95	16	95
2	105	7	115	12	100	17	100
3	110	8	105	13	110	18	90
4	100	9	95	14	90	19	110
5	105	10	90	15	110	20	105

When the cord was simply held before the man :

Order.	lbs.	Order.	lbs.	Order.	lbs.	Order.	lbs.
1	90	6	100	11	90	16	90
2	105	7	110	12	90	17	90
3	105	8	100	13	100	18	85
4	90	9	90	14	85	19	100
5	95	10	85	15	105	20	100

Results.

These two tables show, that men have less power in drawing a cord before them than when they make it pass over their shoulders : it shows us also that the largest men have not also the greatest strength to hold, or to draw in a horizontal direction by means of a cord. To obtain the absolute velocity of these twenty men, I proceeded as follows :

Marching of men for considerable times.

Having measured very exactly a distance of 12,000 Rhinland feet, in a plain nearly level, I caused these twenty men to march with a good pace, but without running, and so as to continue during the space of four or five hours. The following is the time employed in describing this space, with the velocity resulting from each of them.

Ord.	Time.	Veloc.	Ord.	Time.	Veloc.	Ord.	Time.	Veloc.
1	40′ 18	4′ 94	8	40′ 9	4′ 99	15	36′ 17	5′ 51
2	41 12	4 85	9	40 20	4 96	16	41 28	4 82
3	39 8	5 55	10	40 51	4 90	17	42 25	4 71
4	39 40	5 04	11	56 17	5 51	13	40 19	4 98
5	34 19	5 83	12	38 11	5 24	19	39 57	5 01
6	35 11	5 68	13	38 5	5 25	20	37 51	5 29
7	38 7	5 25	14	37 1	5 40			

It is necessary to mention, with regard to these experiments, that I took care to place, at certain distances, persons in whom I could place confidence, in order to observe whether these men marched uniformly and sufficiently quick without running.

Having thus obtained, not only the absolute force, but the absolute velocity also, of several men, I took the following method to determine their relative force.

The same with an additional resistance against the men.

I had made use of a machine composed of two large cylinders of very hard marble, which turned round a vertical cylinder of wood, and moved by a horse, which described in its march a circle of ten Rhinland feet. This machine appeared to me the most proper to make the following experiments, which serve to determine the relative strength that the men had employed to move this machine, and which I use hereafter to determine which of Euler's two formulæ ought to be preferred.

To obtain this relative force, I took here the same pulley which served me in the preceding experiments, by applying a cord to the vertical cylinder of wood, and attaching to the other end of this cord, which entered into an open pit, a sufficient weight to give successively to the machine different velocities.

Having applied in this manner a weight of 215lbs. the machine acquired a motion which, after being reduced to an uniform motion, taking into account the acceleration of the weight of the friction, and of the stiffness of the cord, gave 2·41 feet velocity; and having applied in the same manner a weight of 220lbs. the resulting uniform motion gave a velocity of 2·47 feet. I only mention these two limits, because they serve as a comparison with what immediately follows. I began these experiments with a weight of 100lbs. and increased it by five every time, from that number up to 400lbs.

I made this machine move by the seven first of my workmen, placing them in such a way, that their direction remained almost always perpendicular to the arm on which was attached the cord which passed over their shoulders in an almost horizontal direction.

Results.

Thus situated, they made 281 turns with this machine in two hours, which gave for their relative velocity $c = 2{\cdot}45$ feet per second. We have also the absolute force, or P, from these seven men by the above table = 730lbs. and their absolute velocity, or $C = 5{\cdot}30$ feet.

Therefore, by substituting these values in the first formula, we find the relative force $p = 205$lbs. which agrees very well with what we have just found above.

If instead of this first formula, the second be taken, it gives $p = 153$lbs. which is far too little.

By this it is evident, that the first of Euler's two formulæ is to be preferred in all respects. I have also made a great number of combinations, and I almost always found the same effect.

Euler's first formula preferred.

Dividing the 205lbs. which we have just found, by seven, the number of workmen, we get 29lbs. for the relative force, with 2·45 feet relative velocity for each man, which is rather more than the values commonly adopted in the computation of machinery. A number of other observations on different machines, which I intend to relate another time, have given me the same result; that is to say, we must value the mean human strength at 29 or 30lbs. with a velocity of 2½ feet per second.

A man's strength 29 or 30, with 2½ ft. veloc. per second.

To obtain the ratio of the strength of a horse to that of a man, I had the same machine moved by a horse, without altering any thing; and I found by ten different horses which I used successively, that a horse makes 603 turns in two hours instead of 281; therefore, by supposing the static motion of a horse seven times greater than that of a man, we find that the former has 5·3 feet per second of velocity.

By this it is evident, that the effect of a horse is fourteen times greater than that of a man, or, which amounts to the same thing, fourteen men must be used instead of one horse. Hence it appears, that it is much more advantageous to employ horses than men in moving machines, if other reasons did not require us to prefer men.

Horses exceed men 14 times in drawing.

I have also made a number of other interesting observations on horses and oxen, which are likewise used in moving machines; but as I am now waiting for observations of this kind, which other persons are making according to my plan, I shall reserve them for another memoir.

II.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature.

By Professor J. BERZELIUS.

(*Continued from p.* 166.)

Indication of experiments made upon the combinations of metals with oxigen and with sulphur for determining their composition, their electro-chemical nature, and relations.

AFTER this general review of the changes which appear to be necessary in the theory of chemistry, I shall have the honour to present to the academy the results of some experiments upon the combinations of various metals with oxigen and with sulphur, made with the intention partly of determining their composition with greater precision, in order to refute certain incorrect notions respecting their nature, and partly to ascertain the electro-chemical nature of those metals, as well as the place they ought to occupy in the system among the other combustibles. Much remains yet to be done on this subject, because the field to be explored is so extensive, that each individual step appears relatively of small magnitude. My researches have been made upon the oxides of tin, tellurium, gold, platina, palladium, lead, zinc, and manganese; and at my request the following metallic oxides have been analysed, namely, those of cerium by M. de Hisinger; those of nickel and cobalt by M. Rathoff; that of bismuth by M. Lagerhjelm; and those of mercury by M. Sefftroud; and these chemists solicit the honour to publish their works in the Memoirs of the Academy.

Ammonium?

I should also have wished to add to these experiments that of the production of an amalgam of ammonium produced an anhydrous ammoniacal salt; and though in my experiments on that subject, an amalgam of kalium has produced an amalgam of ammonium in the subcarbonate of ammonium, prepared with the carbonic acid gas and dried ammoniacal gas, I shall not venture to present the same to the Acadamy as a well-determined result, because I have not yet had an opportunity of examining to what degree I may have succeeded in operating with materials perfectly deprived of water. It is, nevertheless, clear, that the success of an experiment of this nature would be decisive as to the nature of ammonium.

I. Concerning the Oxides of Antimonium.

On the number, &c. of antimonial oxides.

Notwithstanding the labours of chemists have, perhaps, been more frequently employed upon this metal than upon any other, we have hitherto possessed but few data respecting the number and the nature of its oxides; and the information given in elementary works is often contradictory the one to the other. The chemists who have operated the most successfully upon these oxides are, MM. Proust, Thenard, and Bucholz. Thenard, guided by the principle of unlimited combinations advanced by the illustrious Berthollet, found that antimony produced six different oxides, that is to say, one black, one chestnut brown, one greyish white and fusible, one white and not fusible, one orange, and one yellow, in which the quantity of oxigen differed no more than one or two per cent. Proust, on the contrary, found no more than two oxides, of which he has determined the composition with considerable accuracy; and Bucholz, who repeated the experiments of Thenard with the intention of examining them, could find only two degrees of oxidation precisely the same which Proust had described. I have found as many as four, which it is incontestible that Thenard saw, though he did not well distinguish them from the mechanical mixtures of different degrees of oxidation, which are very frequently obtained; and though he has given no other distinctive character than the colour, which is so often fallacious.

The author has found four.

Antimony was purified by fusion with its oxide.

I must observe, that the antimony employed in all my experiments was purified in the following manner: I reduced it to powder, and mixed it with the white oxide of antimony, which I then exposed to fire till the mixture was fused. If the fused oxide, which flowed above the metallic bottom, was found to be coloured after cooling, I repeated the same operation.

1. Suboxidum stibicum; formed in the atmosphere; blackish grey.

1. *Suboxidum stibicum* is formed when the metal is exposed for a long time to the action of a humid and warm atmosphere. It forms an extremely thin coat of a blackish grey colour, which prevents all farther action of the atmosphere upon the parts so covered. In order to obtain this suboxide in

larger quantities, a piece of antimony, fused in a tube of glass, to give it a convenient form, was employed as the positive conductor in the decomposition of pure water by a voltaic pile of fifty pair. The antimony produced oxigen gas in extremely small bubbles, but, at the same time, it became covered with a grey pellicle which became almost black when the metal was dried in the air. That part of the antimony which was covered by the cork had preserved its metallic brilliancy, and the difference between the suboxided surface and clear metal, was very marked. But as even in this experiment the suboxide did not appear visibly to increase as soon as the pellicle was formed, I employed antimony reduced to powder as the positive conductor, and touched at the bottom by the point of the platina wire. This point produced oxigen gas, which, from time to time, rose through the powder, and this last began to be covered with a lighter and bluish powder. After some days this powder had increased so much as to be capable of being separated from the metal by means of levigation. This becomes nearly black by drying, and, when rubbed with a polished bloodstone, did not give the smallest trace of metallic brilliancy. When thrown into muriatic acid, this fluid emitted a slight smell of hydrogen, and a few instants afterwards, metallic particles were seen swimming in the acid, and were more easily precipitated by soda than the powder before the action of the acid. The suboxide of antimony, therefore, possesses a property common to most of the suboxides, of being decomposed by the action of acids, by concentrating the oxigen upon part of the metal to produce a base combinable with the acid, and reducing the other part to the metallic state.

Also by voltaism.

Another voltaic process on the antim. powder.

Decompos. of the suboxide by an acid.

Composition not determined by the exp.

I have not been able to produce this suboxide in a sufficient quantity to analyse it; but I shall hereafter shew how it is possible to find its composition by calculation with some degree of probability.

2. Oxidum stibiosum. dull white, fusible, &c.

2. *Oxidum stibiosum.* The characters of this are very well known from the experiments of Proust and Thenard. It has a dirty white colour, is slightly soluble in water, is easily fused by a cherry red heat into a yellowish fluid. The mass, when cold, is crystallized in the manner of asbestos, but the groups of crystals cross in every direction, and it is not difficult to break their continuity.

a. In order to determine the quantities in the composition of this oxide, I digested ten grammes of antimony with nitric acid until they were completely corroded. I then mixed the liquid with much water, and washed the precipitate with water until that fluid came off without being capable of reddening turnsole. The oxide thus obtained weighed, when dry, 12·005 grammes; and, in order to drive off all water, I exposed it in a glass capsule to an heat not as high as ignition, but it took fire on a sudden, and continued to burn like fungus, (or tinder) at the same time subliming in a thick white smoke, of which part was condensed on the sides of the glass. The powder, by this means, became as white as snow, and weighed 12·3 grammes.

Formed with antimony corroded by nitric acid.

Takes fire by low heat, and becomes very white.

b. As this analytical method did not appear very good, I mixed in a small glass retort ten grammes of murias hydrargyricus (corrosive sublimate) in powder, with twenty grammes of powdered antimony. The atmospheric air of the retort having been expelled by hydrogen gas, and a small receiver filled with hydrogen gas being also applied, I gently heated the mixture until the murias stibiosus (butter of antimony) came over, and lastly I heated the body of the retort red hot, to distil over the mercury amalgamated with that part of the antimony which had been added in excess. The quantity of 16,98 grammes of antimony remained in the retort. Consequently ten grammes of murias hydrargyricus had been decomposed by 3·02 grammes of antimony. But 100 parts of this salt contain 5·75 of oxigen combinable with other metals. 100 parts of antimony had, therefore, been combined with thirteen parts of oxigen.

Another method by heating antim. and corros. sublimate in hidrogen;

and from the quantity of the latter decomposed by a known quantity of antim. the components of the oxide were deduced, viz. 100 parts antim. and about 19½ oxigen.

I repeated this experiment several times without having ever obtained results perfectly equal, but varying, as for example, 19,35 or 19,68 parts of oxigen for 100 of antimony. The causes of error in this experiment may be several. For instance, it is possible that the mercury may be so adherent to the antimony as not to be separated but at a temperature which would also carry over a little of the latter; and it is also possible that a small quantity of mercurial muriate may arise before decomposition along with the vapors of the muriate of antimony. It is, therefore, probable, that these experiments may have given the quantity of oxigen rather too great.

Indirect method of deducing the components of an oxide.

In my essays on determinate proportions, I have often ascertained the composition of an oxide, which was difficult to analyse with exactness, by analysing the sulphuret of the same metal, and calculating the composition of the oxide from this analysis. I endeavoured to do this in the present case.

Sulphuret of antimony was made by heating antimony and cinnabar.

Cause of inaccuracy.

Inference that 18·6 parts oxigen and 100 antimony form this oxide.

3. *Sulphuretum stibii.* I mixed 100 parts of pulverized antimony with 500 parts of very pure cinnabar, and I exposed the mixture to heat in a retort. When the cinnabar appeared to be entirely decomposed, and the excess driven out of the bulb of the retort, I left the sulphuret of antimony in fusion for several minutes at a cherry red heat, and then took the retort from the fire. The sulphuret of antimony weighed 137·3 grs. In the upper part of the retort I found a small quantity of a reddish substance sublimed. I supposed it to be cinnabar not completely expelled, and heated the sulphuret again in the retort till it boiled. The red substance was increased, and I at last discovered that it was crocus of antimony produced by the access of air. As the sulphuret of antimony is slightly volatile in a very elevated temperature, the result of this experiment likewise is not very exact; but it may, however, be inferred, that the quantity of oxigen in the oxidium stibiosum cannot be less than 18·6 for one hundred parts of metal.

4. White oxide. (*a*) Antim. oxided by nitr. ac. and ignited.

4. *White oxide of antimony**. (*a*) Two parts of pulverized antimony oxided (in a phial carefully weighed) by pure nitric acid, and the oxided mass strongly ignited in the phial, produced in different experiments 125·8, 126·13, and 127·8 parts of white oxide.

(*b*.) Antim. dissolved in nitro-mur. acid, and precipitated by water and ignited.

Deduction, 100 antim. and about 26 oxigen, *which is nearly 1½ times the oxigen in the second oxide.*

b. 100 parts of pulverized antimony first dissolved in nitro-muriatic acid, and then precipitated and well washed with water, produced a quantity of oxide of antimony, which, after strong ignition, weighed 126·56 parts. The acid liquor, after dilution, contained no more oxide, and did not become turbid by saturation with an alkali. The experiments appear, therefore, to prove, that this oxide does not contain less than 25·8 nor more than 27·8 of oxigen to 100 parts of metal. We see, therefore, that this oxide must contain 1½ times as much oxigen as the preceding degree, though the exact number was not ascertained.

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* I shall hereafter explain why I do not here use the words *oxidum stibicum*.—B.

Exp. The oxide of the first degree was heated with the metal (antim.) the combination was fusible, &c.

In order to obtain a more determinate result by another process, I endeavoured to reduce the white oxide at the first degree of oxidation by means of metallic antimony. I, therefore, mixed the metal in extremely fine powder, with less of the oxide than would have been required to oxide the metal. I introduced the mixture into a small phial, of which I drew out the neck into the form of a capillary tube. The body was bedded in sand in a small crucible, and exposed to a sufficiently strong fire to make it red-hot; and, at the moment when the matter entered into fusion, I hermetically closed the end of the capillary neck, by melting the extremity, and I left the mixture in that heat for half an hour. Three grammes of the white oxide of antimony had oxided 0·323 grs. of metallic antimony, and afforded a fusible oxide. I reduced this again to powder, and mixed it with powdered antimony, after which I fused it in a similar phial; but in its present state it was capable of dissolving only a small quantity of antimony, which would have been correspondent with 0·03 grs. of antimony upon the whole quantity of oxide. The fused oxide which I had obtained by this operation, was of a pearl colour, its fracture crystalline, granulated, and very compact; it was extremely coherent, and difficult to break, and all its external properties proved that it was not a pure stibious oxide. I repeated this experiment several times, and always found that the white oxide, fused with metallic antimony, dissolved one-third more than it before contained; that is to say, that 100 parts of the oxide can oxide an addition of 26 parts of the metal; and if we attend to this result, which is determined with the utmost possible accuracy, we shall find that the fusible oxide produced, of which the external characters are so different from those of the oxidum stibiosum, cannot be the same as this last; but that it must be a combination of the oxidum stibiosum with the white oxide in such proportion that the oxigen of the former is double that of the latter. Such combinations between oxides of different degrees of the same metal, are not very rare, though they have not hitherto been much attended to, or else have been considered as different degrees of oxidation of the metal. I reduced the oxide into powder,

100 parts oxide had oxided 26 more of metal,

forming a compound of two distinct oxides of different degrees.

and digested it with the surtartrate of potash: the stibious oxide was dissolved, while the white oxide was separated in the form of an extremely white and voluminous powder.

Exp. To shew the proportions of sulphur and of oxigen which can separately unite with a definite quantity of antimony. White oxide and sulphur were heated together.

In order to compare the quantity of oxigen in the white oxide with the quantity of sulphur which can combine with the metal it contains, I mixed 100 parts of white oxide with 100 parts of sulphur, and heated them together in a small phial exactly weighed, which had a narrow neck, and of which the aperture was closed with a stopper of charcoal. The heat employed was at first very gentle, until the disengagement of sulphureous acid had ceased, after which I heated the phial amidst the burning charcoal until the uncombined sulphur was entirely dissipated, and the buton of sulphuret remained in fusion at the bottom of the phial. It weighed 107·25 p. Although the volatility of the sulphuret of antimony could not be perfectly obviated in this experiment, I have reason to think, that the length and smallness of the neck of the phial prevented any loss, and more especially as the mass was never so much heated as to put it into a state of ebullition*.

(*To be continued.*)

III.

A Reply to Don JOSEPH RODRIGUEZ'S *Animadversions on Part of the Trigonometrical Survey of England. By* OLINTHUS GREGORY, LL. D. *of the Royal Military Academy, Woolwich.*

To W. Nicholson, Esq.

DEAR SIR,

Surprise on seeing an attack on the English na-

WHEN I say that I have been greatly surprised to see in the second part of the Philosophical Transactions for 1812, Don Rodriguez's animadversions upon part of the English Trigonometrical Survey, I conjecture that I am merely describing a feeling which has been more or less experienced by every man of science in the kingdom. The publication of an attempt by a *foreigner* to cast discredit upon a great national undertaking, in the transactions of the most eminent philosophical institution of that nation, the Royal Society,—that is, in a work which learned men on the continent contemplate as a fair picture of the science and genius of England, is, I believe, a thing unprecedented in the history of literature. If the great work which Don Rodriguez has taken upon himself to examine, had been really reprehensible, it would still have been extraordinary that he should have been permitted to give his censures currency in such a vehicle; but how much more extraordinary must it be thought, if, on inquiry, it shall appear, that his strictures are causeless, and therefore unjust. This is an inquiry which every man of competent information, who has at heart the honour of his country, has a right to institute; and, however unpleasant the undertaking may, in some respects be, I enter upon it without delay, because *Colonel Mudge*, whose reputation is so deeply implicated in this business, is at present prevented from giving Don Rodriguez's paper that decided and complete refutation which it will hereafter receive at his hands, and because his silence, though unavoidable, may be construed into defeat.

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* I never operated upon a substance which in general afforded me results so various as antimony and its oxides. In order to ascertain whether the oxides, which are often obtained with less oxigen than they ought to contain, have been volatilized with the acid employed for their oxidation, I heated such an oxide with sulphur, and converted it into sulphuret. 100 p. of antimony produced 128·5 of yellow oxide, which by ignition left 125·8 of white oxide, and these produced with sulphur 137·5 of sulphuret. It appears, therefore, that these oxides contain combinations of different degrees of oxidation, which prevent the saturation with oxigen.

tional survey in Phil. Trans.

Impressed by these considerations, I propose, in this communication to show, that the observations of this ingenious foreigner are, on all his main positions, unfounded: and, although the matter under investigation is, in general, so nearly elementary that any man of moderate scientific attainments might safely rest the truth of his assertions upon his own character and their intrinsic evidence; yet, lest it should be apprehended that, on this occasion, my judgment may be warped either by strong national feeling, or by private attachment, I shall fortify my positions, as I go along, by such authorities as neither Don Rodriguez, nor any other person, will be inclined to question.

Dr. G.'s reasons for quoting authorities.

Before I proceed to the points which Don Rodriguez selects as the basis of his animadversions, it may not be thought improper if I briefly advert to what appears his main, if not his sole, object, in making those animadversions at all. I shall

The Don's *apparent* object to exalt the French scientific character,

at the expence of all others.

(See also Nich. Journ. New Series, Nos. 26, 27, or vol. VII.)

English Trig. Survey commenced before the French revol.

not, I hope, be deemed uncandid, if I say, that to me this object *appears* to be no other than the depression of English (and perhaps other) ingenuity and exertion, in order to the undue exaltation of the French scientific character. To this end, as it would seem, (for to what other purpose can it be?) we are told, that in consequence of "the general impulse which the human mind received" from the French revolution, the members of their Academy of Sciences "invented new instruments, new methods, new formulæ," for the purpose of ascertaining the figure of the earth, &c. and commenced "an important undertaking, almost the whole of which consisted of something new in science." I have no wish to depreciate the value of the discoveries and improvements of the French mathematicians; yet surely I may affirm, that much had been done with respect to the grand topic in question, long before the French revolution. Did not *Euler* invent "new methods and new formulæ" for this express purpose, and publish them so long back as the year 1753, in the Berlin Memoirs? Did not *Dionis du Sejour* much improve this branch of analytical theory? Did not Professor *Playfair* solve the general problem in all its *useful* varieties in the Edinburgh Transactions, before the publication of Delambre's investigations? Did not *General Roy*, and the subsequent English measurers, publish ingenious formulæ in the Philosophical Transactions, although Don Rodriguez insinuates, that their methods are kept back? And, with respect to actual admeasurements, might not the Don have learnt from the Philosophical Transactions (see vols. 75, 77, 80, &c.) that government surveys were commenced in Scotland so long back as 1745, by Lieutenant-General Watson; that in 1775 the work was continued; that in 1783 an authorized committee or deputation of the mathematical philosophers of England and France met at Dover to concert the best means of carrying a series of triangles from Greenwich to Paris; that the work was soon after pursued by the appointed persons in both countries; and that, from that period it has almost regularly proceeded in England, whatever interruptions it may have experienced in France? How, then, can a writer insert in the Philosophical Transactions, where evidence to the contrary *abounds*, a paper from which all who are unacquainted with the history of this important class of operations, would conclude, that they *originated* in the determination of the French to " establish a new system of weights and measures."

To the same end, apparently, tends the Don's assertion, that " the Swedish Academy of Sciences, *encouraged by the success of the operations conducted in France*, sent also three of its members into Lapland to verify *their* former measurement." For the natural tendency of this statement is to produce the belief, that the recent operations of the Swedish philosophers were in humble imitation of the French, and that they were undertaken for the purpose of verifying, or of correcting, *their own* former admeasurement; in both which respects the colouring given is widely different from the truth. The Lapland measure in 1736 was not conducted by Swedish, but by *French* academicians; and the correction of it was proposed long before the French revolution. The following are the true circumstances of the case, as I received them from a learned Swede. *Melanderhielm*, the venerable president of the Stockholm academy, had almost from his youth doubted the accuracy of the operations of 1736, and sought anxiously for an opportunity of repeating them; but waited many years before he could avail himself of a favourable conjuncture of circumstance, although latterly he had found in M. *Svanberg*, a young man of great talent and activity, to conduct the operative part. After hearing of the new measure of a degree by MM. *Delambre* and *Mechain*, he wrote to some of the French mathematicians on the subject, but with no intention of soliciting them to visit Lapland. Soon after this, Buonaparte, at the suggestion of the then national institute, *wrote* a letter personally to the late king of Sweden, requesting permission for some members of that body to proceed to Lapland, in order to determine an arc of the meridian. That high-spirited young monarch replied, that he would consult his own Academy of Sciences at Stockholm, whether such an operation was desirable for the interests of science; and if they were of that opinion, he had no doubt he could find *Swedish* mathematicians competent to the undertaking. Hence MM. *Svanberg, Ofverbom, Holmquist, and Palander*, were appointed to examine and repeat the measure of the French academicians; and this is what Don Rodriguez terms the expedition of *three* of the Swedish academicians " to Lapland to verify *their* former measurement."

History of the New Lapland measure,

and of Buonaparte's letter to the king of Sweden.

With

Col. M. commended by Don. R. *merely* as a skilful observer.

With the same spirit, it is natural to suspect, Don Rodriguez speaks of Colonel Mudge as "*a skilful observer*," and merely such; adding, that "one cannot but admire the beauty and perfection of the *instruments* employed" by him: while, when he characterises the labours of the French measurers, he assures us they "merit the *highest* degree of confidence," and, "by the sanction of such an union of talents, give such a degree of credit and authenticity to *their* conclusions, as could scarcely be acquired by other means." I shall not animadvert upon this invidious contrast; but simply remark here, that the Don adopts a strange method of verifying his positions. He admits, that Colonel Mudge is a *skilful observer*, who knows very well how to employ his instruments; and, that there may remain no doubt on that head, publishes a long paper to prove, or at least to show it probable, that he has made a *mistake* of $4\frac{1}{2}$ seconds in the determination of a zenith-distance. This animadverter has, as he assures us, gone through *all* the Colonel's computations by different processes, and found them correct, or only evincing very trifling discrepancies, such as may naturally arise from the diversity of methods; yet he cannot find in his heart to drop a single word of commendation on him as a computer or as an investigator.

The preceding remarks will suffice, I apprehend, to render manifest the probable object of Don Rodriguez's paper. I shall now proceed to enquire how far the reasons assigned by this gentlemen bear him out in his attempt to throw suspicion upon the operations of Colonel Mudge, in measuring an arc of the meridian. The Don's paper, it is true, is rather desultory and unconnected; but, I trust I shall neither misrepresent him, nor do injustice to his arguments, by endeavouring to reduce them to the following order:

Don. R.'s first reason for questioning the accuracy of the English observations refuted.

1. Colonel Mudge's observations must be wrong somewhere, because his results do not correspond with those of the French measurers. This is not positively affirmed, but every where strongly implied: for Don R. assumes his value of the radius of the earth's equator from the French measurements and computations; and he takes it for granted, that the fraction exhibiting the ratio of the difference of the earth's axes to the major axis, technically termed the *compression*, lies somewhere between those limits, ($\frac{1}{310}$ and $\frac{1}{310}$,) which a superficial ob-

server would adopt as most suitable to the French operations. Such assumptions, by the way, are neither consistent with fair criticism nor with sound logic: for the grand object in measuring arcs of meridians is to DETERMINE *the ratio* of the earth's axes; and when, in the course of any such admeasurements, avowedly remarkable anomalies arise, it is a mere *petitio principii* to conclude that there *must* be some error in the astronomical *observations*, because irregularities as great or greater than those which the operations indicated, result from computations resting upon a gratuitously assumed ratio.

The French operations indicate *greater* irregularities.

But some of the French operations at home, compared with those at Peru, give about $\frac{1}{309}$ for the compression*. Be it so. That is no reason why any such ratio should be adopted, as the test by which to try the accuracy of English *observations*. Don Rodriguez himself, when applying the same test to the French meridian, thereby detects irregularities, and great ones too; yet does not whisper the gentlest hint that they were occasioned by inaccurate observations. Why not? Because M. Mechain "handled instruments with *great* delicacy, and was possessed of *peculiar* talents for this species of observation" So that a gratuitous assumption should suffice to render English observations doubtful, while it leaves the accuracy of French ones unimpeached. To me it appears that a candid critic would, in analogous circumstances, make analogous inferences; and not sift one class of results to the bottom, while he satisfies himself with merely glancing at the surface of the other class. Had he examined the French measures a little more minutely, he would, instead of adopting them as his standard, have found that they exhibit far too great irregularities to be entitled to that honour. Taking the results of the operations of Delambre and Mechain, as subdivided naturally by the assumed stations at Dunkirk, the Pantheon at Paris, Evaux, Carcassone, and Montjouy, and applying to them the principle developed by Legendre, in which, "the sum of the *squares* of the errors is made a *minimum*," the requisite compression is $\frac{1}{148}$; and even then the deviations from what the theory would require are, at Dunkirk $-2''\cdot23$, that is, nearly $2\frac{1}{4}$ decimal seconds; at the Pantheon, $+5''\cdot63$; at Evaux, $-4''\cdot79$; and at Carcassone,

Some anomalies in the French measures.

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* Biot. Astronomie Physique, tom. i, p. 159.

+1″·34. Here the compression which agrees best with the observations is more than *double* what it ought to be. If a medium compression had been chosen, the errors at the several stations would have deviated still farther from the probable errors of observation. Don Rodriguez will find this confirmed by Puissant, *Géodésie*, pa. 137. 141, and by Laplace, *Exposition du Systême du Monde*, Liv. i. ch. 12. After he has duly reflected upon the deductions of those philosophers, he will, perhaps, be convinced, that he has been rather precipitate in taking the French operations as a standard.

Don R.'s 2d reason refuted.

But, secondly, this writer infers that there must be some error in Colonel Mudge's observations, because they tend to shew that the terrestrial spheroid is very irregular. All the measurements "which have been hitherto made in the northern hemisphere, are (he tells us) *extremely satisfactory by their agreement*, and give us great reason to presume, that the general level of the earth's surface is elliptical and *very regularly so*." "There would not have remained the smallest *doubt* respecting the earth being flattened at the poles," but for the "measurement performed in England." But "this measure alone would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part (the author means *those parts*) than at the equator, or at least, that its surface is *not that of a regular solid*." The degrees, in fact, increase as the latitudes diminish, which, says Don Rodriguez, "excites a suspicion of some incorrectness in the observations themselves;" whereas, the only fair inference is, that an *insular* situation is very ill fitted to promote the determination of the figure of the earth. Let us see, however, how "*satisfactory*" former measures have been "*by their agreement*," and how completely they prove that the earth's surface is "*very regularly*" elliptical. Lacaille's degree in lat. 45° N. compared with Bouguer's at the equator, gives for the compression $\frac{1}{288}$. The degree in Maryland, with Bouguer's equatorial, gives $\frac{1}{500}$. The Spanish degree at the equator, with the French degree lat. 45°, gives $\frac{1}{301}$. Boscovich's Italian degree, lat. 43°, compared with Bouguer's at the equator, gives $\frac{1}{303}$. Bishop Horsley, by a geometrical mean of twelve different ellipticities, obtains $\frac{1}{206 \cdot 4}$. Boscovich, taking a mean from all the measures of degrees, so as

Former measures do not agree in their results, as Don R. affirms.

Summary of statements,

as to make the positive and negative errors equal, obtains $\frac{1}{348}$. Lalande, by comparing Father Leisganig's degrees in Germany with eight others in different latitudes, gets $\frac{1}{311}$. And the recent measures in France give, as we have seen, $\frac{1}{148}$. Such is a summary of the evidence from which it is to be concluded that the earth is "elliptical," even "very regularly so." General Roy, who had got a habit, not very uncommon among scientific Englishmen, of deducing reasonable conclusions from anomalous appearances, and not twisting them to suit a fanciful hypothesis; assumed *seven* different spheroids of varying ratios between $\frac{1}{179}$ and $\frac{1}{540}$, and, on finding that none of them corresponded so uniformly as might be wished, with the operations in different latitudes, made these inferences: "Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not *any way correspond* with the measured portions of the surface of the earth." "Hence it is that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has *no regular figure*, that is, not such as would be generated by the revolution of a curve around its axis." And again, after specifying some other facts, "from all which we may conclude, that the earth is *not an ellipsoid*."

and General Roy's deductions from them.

Nor is this opinion peculiar to General Roy, it is common, I believe, to all who have contemplated the subject, except Don Rodriguez. Thus, Puissant, at p. 187, of his *Géodésie*, says "La comparaison des divers degrés mesurés à l'èquateur, en France, en Pensylvanie, etc. donne lieu à décider que les méridiens sont différens entreux et n'ont pas la forme elliptique." And at p. 222. "D'on l'on doit conclure que la terre *n'a point* la forme régulière que l'on serait tenté de lué attribuer." To the same purpose writes Laplace, at p. 56 of his "*Exposition*:" "Les degrés du nord et de France donnent $\frac{1}{146}$ pour l'ellipticité de la terre, que les degrés de France et de l'équateur, donnent égale à $\frac{1}{334}$; il paroit donc que la terre est sensiblement différente d'un ellipsöide. Il y a même lieu de croire *qu'elle n'est pas un solide de révolution*, et que ses deux hémisphères ne sont pas semblables de chaque côté de l'équateur.

Puissant's deductions

Laplace's

It is curious, however, to observe that, notwithstanding this extreme want of uniformity, in the results furnished by terrestrial

trial admeasurements, those which are deduced from astronomical theory, and the oscillations of pendulums, correspond very nearly. Thus, Laplace's deduction of the compression from the lengths of pendulums in different latitudes, is $\frac{1}{335.78}$. (See Puissant, *Topographie*, &c. p. 66.) Clairault's well known modification of Newton's theorem, derived from the diminution of gravity, gives $\frac{1}{304}$. The phenomena of the precession of the equinoxes and the nutation of the earth's axes, give $\frac{1}{304}$ for the maximum limit. A lunar inequality in longitude depending upon the earth's ellipticity, and expressed by — 20″ 987 sin. ☊ of the moon in longitude, requires the compression to be between $\frac{1}{334}$ and $\frac{1}{305.05}$, but nearest the latter limit. And a lunar inequality in latitude, depending also on the compression, and expressed by — 24″ 6914 sin. ☽, requires the compression to be between $\frac{1}{334}$ and $\frac{1}{304.6}$, still leaning to the latter limit. So that the ratio of the earth's axes, as deducible from these independent theoretical considerations, lies within much narrower limits than we can get in any other way. But this does not affect the truth of the preceding remarks. It serves principally to shew, that whatever may have been the derangements of the terrestrial spheroid since its original formation, they are not such as have differently affected the several phenomena occasioned by its aggregate attraction: while a very slight consideration of the effects of the deluge, of earthquakes, of volcanic operations, of extensive dislocations of strata, &c. may serve to convince us, that, however regular the earth might once have been in its general shape, there is now no reason to expect that "very regular" surface from which Don Rodriguez persuades himself there ought to be no essential deviation.

The only inferences which agree are those from pendulums, and astronomical theory.

Remarks on this fact.

A 3d reason of the Don's refuted.

3. Don Rodriguez is farther confirmed in his opinion, that there must be an error in the observations, especially at Arbury Hill, of "nearly 5 seconds," because he thinks no such anomaly as that can fairly be ascribed to the effect of local attractions. He does not deny "that irregularities of the earth and local attractions may occasion considerable discrepancies;" yet he does not believe they can ever produce a deviation of the magnitude just specified. Here again he is at war with the decisions, I believe, of all preceding philosophers who have

directed their attention to this subject. There are, obviously, three causes which may jointly or separately occasion a deflection of the plumb-line from the true perpendicular to the earth's surface; namely, an insular situation, the attraction of mountains, and strata of unequal density beneath the surface; and either of these may be productive of considerable effects.

Three causes of a deflection of the plumb-line.

To arrive in the easiest manner at an estimate of the effect upon a plumb-line arising from observations made in an insular situation, let Don Rodriguez imagine the simple case of a triangular island so posited on the surface of an aqueous spheroid, that a meridian shall run along from its vertex, directed northward to the middle of its base: he will perceive that, in such a case, as an observer proceeded from the south towards the north, there would be a constant variation in the deflection of the plumb-line; in such manner, that there would be only one point on the meridian, where the attractions occasioned by the island itself should be so counterpoised and adjusted, that the true and observed vertical lines should correspond. Pursuing this hypothesis, with the requisite modifications for a neighbouring continent on the south, and an immense ocean north, he will find that the singular order exhibited by the English estimates of degrees, though an unexpected, is by no means an unnatural, consequence of our insular situation. Dr. Hutton has treated this very point with his usual perspicuity, in a valuable note at page 198, vol. ii. *New Abridgment of the Philosophical Transactions*, published in 1803. That note is too long to be copied into this place; I shall, therefore, merely transcribe the Doctor's concluding inference: "Hence also it follows, that insular situations must be worst of any; having the plumb-line deviating to the north at the south end of the line, to the south at the north end, to the east at the west side, and to the west at the east side; thus producing errors in all observed latitudes and longitudes."

Dr. Hutton on the deflection from insular situations.

Laplace, most probably alludes to this kind of effect, at p. 59, "Exposition," where he speaks of the *much more extensive attractions than those of mountains*, of which the effect is sensible in Italy, *England*, &c.

Laplace ditto.

That the deflections of the plumb-line, and the consequent estimate of the lengths of degrees, must be greatly affected by hills and valleys, is also very manifest. Professor Playfair,

after

Playfair on the attraction of hills, &c.

Maskelyne.

Cavendish.

after describing the irregularities thus occasioned in the degree at Turin, adds, "there are, *no doubt*, situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian, *infinite, or even negative.*" See *Edinburgh Transactions*, [illegible]. v. p. 5. And Dr. Maskelyne, after treating of Mason and Dixon's degree in North America, says, "Mr. Henry Cavendish having investigated several rules for finding the attraction of the inequalities of the earth, has, upon probable suppositions of the distance and height of the Allegary mountains, from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be so produced in the length of the degree; and finds that it may have been *diminished by 60 or 100 toises* by these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be *very sensibly* affected by the attraction of hills, and defect of the attraction in the Mediterranean Sea and Indian Ocean." *Phil. Trans.* vol. lviii. or *New Abridgment*, vol. xii. p. 578.

Puissant on local attractions.

Playfair on ditto.

With respect to the third cause of irregularity, Puissant, Géodésie, p. 137, remarks, that "anomalies in the latitudes, are, *doubtless*, produced by local attractions which change the direction of the apparent vertical." And Professor Playfair, in the excellent memoir I have just quoted, (a memoir, it should be recollected, which was written *five* years before the remarkable anomalies in the English measures were known) affirms, that "from suppositions no way improbable, concerning the density and extent of masses of varying strata beneath the surface, he has found, that the errors thus produced, *may easily amount to ten or twelve seconds.*" "This cause of error, (as he justly remarks) is formidable, not only because it may go to a great extent, but *because there is not any visible mark by which its existence may always be distinguished.*"

Here, then, are *three* sources of deflection from the true plumb-line, neither of which is correctly appreciable in all circumstances, yet of which each may be not only perceptible, but important; and the concurrent effect of all may, doubtless, be very considerable. Yet, Don Rodriguez is unwilling to attribute a deviation of 4 or 5 seconds, to any, or all, of these causes.

4. This writer infers, that mistakes must have occurred in the observations, because the sum of other "errors will be found in the estimate of the entire arch, and will increase in proportion to the extent of the arc measured; but in the English measuremen[illegible]e find exactly the reverse of this." Here he assumes the principle proposed by Boscovich, but condemned by Laplace, for a reason thus briefly assigned by Puissant:—"La solution donnée d'abord par Boscovich est *vicieuse* en ce qu'elle est fondée sur une *hypothèse inadmissible*, savoir, que les erreurs dans le mesure des arcs du meridien sont proportionelles à leurs longueurs"

Don R.'s 4th reason contradicted by Laplace.

5. He concludes that there must be "an error of some seconds in the observations of the fixed stars," because "the results of the observations made on different stars, differ no less than 4 seconds from each other." Now, what are the facts on which this inference rests? Simply these: that the only two stars which indicate any such difference in the whole series of observations, are μ Draconis and ζ Ursæ; that they give a difference of 4″·19, not in the amplitude of the arc between *Dunnose and Arbury Hill*, but of that between *Dunnose and Clifton*; and that, whether these two stars be rejected, or retained with the other *fifteen* employed in finding that amplitude, they will not occasion a difference of a quarter of a second in the result. How, then, can a fair investigation bring this as a reason for an alleged inaccuracy, when it obviously cannot apply to the case? And what must be thought of his impartiality, if it shall appear, that *even in this respect*, the observations of the French and of Major Lambton, which he so manifestly prefers to the English observations, are far more open to censure? Allow me, therefore, just to make the comparison.

Don R.'s 5th reason of no weight,

because the stars, where he finds the irregularity do not apply to *Arbury*, but to Clifton.

Of the English observations, *none* are suppressed, (the observers going upon the principle explained by Simpson, in his "Tracts," which clearly establishes the propriety, if not the necessity, of taking the mean of a *number* of observations) and yet, no irregularity of consequence, except the one above specified, appears. But, it may be seen from p. 72, *Discours Preliminaire*, tome, i. *Base du Système Métrique Décimal*, that no less than *sixty-eight* of the French observations upon β Ursæ majoris were rejected, and termed *bad*; for no other

Much greater irregularities in the French observations; they evince, besides, a little "*coaxing*."

reason that I can perceive, than, that if they had been employed, they would have given the latitude of Dunkirk about a second less than the observations of the pole star gave it. Let Don Rodriguez reflect upon this, and then repeat that the French operations "*merit the high*[illegible]*gree of confidence.*" But this is not all. From p. 39 of the same *Discours Preliminaire*, it appears that *three* stars only were *selected* by Mechain at Montjouy, in consequence of the coincidence of the results arising from them. Among the stars rejected, was ζ *Ursæ*, because different observations gave a difference of 4″. So that the French also detected an irregularity respecting this star. They assign, however, a wrong reason for the fact; for they attribute it to errors in Bradley's table of refractions, while the truth is, that ζ Ursæ is a *double* star, by no means easy to observe properly. Indeed, it appears, not only from the observations of Col. Mudge, &c. but from those of Dr. Herschel (*Phil. Trans.* vol lxxxii. *New Abridgment*, vol. xv.), that both μ Draconis and ζ Ursæ are double stars; that of the former, the two constituent stars appear equal, both white, and not easily distinguishable, and at the distance of 4″. 35 from each other, mean measure; and that of the latter, the two are considerably unequal, and the largest difficult to bisect. Hence Herschel's observations completely confirm those of our trigonometrical surveyors. See also the catalogues of Wollaston and Bode.

Herschel's observations on double stars, show the reason of the apparent anomaly, in Col. M.'s case, and make it rather a proof of accuracy.

Let us next enquire how far Major Lambton's observations, which Don Rodriguez also seems to delight in eulogizing, deserve to be preferred to Colonel Mudge's. From p. 356, vol. x. *Asiatic Researches*, we learn that the Major's observations upon α serpentis were 14, of which two were 5° 57′ 3″·38 and 5° 56′ 53″·98 furnishing a difference of 9″·3; more than *double* the difference that has been found in the English observations, of which the Don complains! At p. 357, again we have a register of 16 observations upon ν Aquilæ, of which two differ by 6″·77. At p. 358, we have 18 observations upon Atair, of which two differ by 5″·38. There are also some other palpable differences in Major Lambton's results, as deduced from *different* stars. The greatest is between Atair and Markab, being 5″ 48. Atair, from the number and agreement of its observations among themselves, should be correct in zenith

Major Lambton's observations so warmly commended by Don R.; abound in *great* discrepancies.

zenith distance, yet it gives the latitude of the station, Dodagoontah, less by 3'' 4 than the mean of the nine stars, employed by Major Lambton, exhibits it, and the latitude found from a mean of the four northern stars, is 2'' 04 *greater* than the latitude found from [illegible]an of the five southern stars. Discrepancies of more than 4'' may likewise be frequently found in the observations recorded in vol. viii. of the "Researches." Most of them are, probably, in great measure, attributable to the imperfections in Major Lambton's sector, which is only of 5 feet radius (while the English is of 8 feet); and is provided with but few comparatively of the requisite means of adjustment: but whether they are to be ascribed to the observer or his instruments, they prove that Don Rodriguez has been rather precipitate in saying, "the same Major Lambton, who has *succeeded so well* in Asia, and is in possession of such *perfect instruments* for the purpose, would be *singularly qualified* for a similar undertaking in Africa." In matters which admit of examination and proof, it is not the custom with Englishmen to bow at once to the authority of a mere *ipse dixit.* Was Don Rodriguez really ignorant that, with respect to accuracy of observation, the English proceedings are thus greatly superior to those of the French and of Major Lambton? If so, how greatly is he to be pitied for writing so much on a subject he had previously so little considered. If he *was* aware of this superiority, how much more is he to be pitied, for giving so unfair and unnatural a representation of the business before him.

From one or other of the reasons I have thus examined, Don Rodriguez says, "*it is almost beyond a doubt* that it is to errors in the *observations* of latitude," the singularity in Col. Mudge's results must be ascribed. There *must* be an error of some seconds in the observations, "especially at Arbury Hill." And he asks, "How is this to be discovered?" How? Why, by simply *repeating* the observations at Arbury Hill. The position of the station is so clearly described in the Philosophical Transactions, that any person may find it within 20 feet; and the farmer who owns the field, can show the identical spot. Don Rodriguez, or some one of his friends, has, doubtless, *handy* circular instruments of the French construction, by which the zenith distances could readily have been taken, and then the

Why did not the Don recur to the ready test of repeating the observations at Arbury?

correctness or incorrectness of the English observers might have been proved in a way from which there could be no appeal. Though, to be sure, if that plan *had* been adopted, and the English results had, in consequence, been verified, Don Rodriguez's paper could never have ap[illegible]red.

There is, however, a method of determining the point, even without taking this trouble. Having then shown, I trust satisfactorily, that Don Rodriguez's reasons for imputing an error of 4 or 5 seconds to the English observations, are nugatory; I shall now proceed, with all possible conciseness, to show that *there cannot be an error of one second* either in the observations at Arbury Hill, or at Dunnose; and those at Clifton are, by the Don's own concessions, out of the question.

The mode of *fixing* the zenith sector precludes error.

First, the manner of *fixing* the zenith sector could not lead to error; for, "to procure for the external stand (says Col. Mudge, Phil. Trans. 1803) and thence for the whole apparatus, a firm foundation, I caused four long stakes to be driven into the ground, one for each foot of the stand, to which its feet were firmly screwed down. The surfaces of the stakes were cut off smooth, and brought into the same horizontal plane, by which means the interior frame and sector were placed much within the limits of their several adjustments." The whole was enclosed in a suitable observatory.

French observations at Chatillon made during a *high* wind, on a stage so tottering, that even a little breeze much disturbed the observ-

Don Rodriguez may perhaps think the French method of fixing their instruments, on some occasions, preferable to this. The reader shall judge. Their instruments, both for taking horizontal and vertical angles, were sometimes placed on *tottering* stages, so as to give anomalies in the angles from 5'' to 8''; furnishing, as Delambre terms them, "le tourment des observateurs." Thus, at p. 46, *Discours Preliminaire*, we are told that at Chatillon, there was a high wooden stage erected for an observatory, in which the carpenter had so badly done his work, that "le *moindre* vent agitoit toute la machine, de manière non seulement à rendre les observations moins sûres, mais à *inquieter les* observateurs." And on turning to p. 174, tome i. it will be seen that the observers had not to contend with a gentle gale; for they there tell us of the "*Grand vent* qui agitoit le signal et l'instrument." The whole was *blown down* shortly after. Will Don Rodriguez place reliance on observations made from such a platform in such a wind; and, notwithstanding, doubt the observations made with a stable instrument by the English? And let him not forget, that whatever error was thus occasioned in the distance between Boiscommun and Chatillon, is more than doubled in all the remaining triangles of the series, by reason of the bad shape of the triangle, Chatillon, Boiscommun, Chateauneuf.

No error can fairly be ascribed to the *construction* of the sector;

If no error in the English observations can be fairly imputed to the manner of *fixing* the zenith sector, neither can any be ascribed to the "*construction*" of the instrument itself. This was most positively declared by two very excellent judges, the late *astronomer royal*, and the Hon. *Henry Cavendish*, on their close examination of the instrument. It will also be inferred, without hesitation, by all competent judges, on reading the description of it in the Phil. Trans. for 1803. To those who have seen neither the instrument nor the description, it may suffice, if I remark, that the equality of the divisions on the arch, is evinced from this consideration, that on running the micrometer screw from division to division, over the whole arch, there was no where an indication of an error amounting to half a second; and that the instrument still continues free from important "derangement," is tolerably well proved by this, that the line of collimation has been *constant* during all the observations and all the journeyings of the sector, and that it still continues the same.

or to subsequent derangement;

In the next place, it may be remarked, that no error in observation can be imputed to a deviation from "*vertical* position" in the sector. Important inaccuracy, in this respect, is precluded by the great length of the axis, by which the instrument is rectified; and by the ready and certain means of placing the plumb-line directly over the illuminated *dot* which marks the middle of the axis, or true centre of the divided arch. For want of these admirable modes of correction, all previous instruments are necessarily imperfect. It appears from Phil. Trans. for 1803, pp. 405, 406, that when the instrument is adjusted in one position by means of the plumb-line and dot, it is turned to a position at right angles to the former, and the adjustment confirmed; and this being the case in these two situations, the instrument must necessarily be vertical in all others.

or to a deviation from vertical position.

Various reasons may be assigned to show that the sector could

not,

or to a deviation from the plane of the meridian.

not, at any of the stations, be out of the plane of the *meridian*. I shall select only two or three. As 1st, if the sector were inclined to that plane, just so much would the path of any star, in its apparent motion, be inclined to the horizontal wire of the telescope; instead of which, both Colonel Mudge and Captain Colby assure me, that when a star came into contact with the wire, the light of the star would appear on both sides of the wire for about three-fourths of a minute of time, the light on each side being equal at the central wire: which of itself is a positive proof. But, 2dly. had the sector been out of the plane of the meridian, the times of the transits of the extreme stars employed, as compared with two excellent time-keepers, must have shewn it. Farther, the errors arising from a wrong plane of the meridian, being comparatively very great in the extreme stars, and small in those near the zenith, it would follow that the error in Capella, which is almost at the extremity of the arch, would be great, compared with those in β. Draconis, κ Cygni, &c. which were within a small distance of the zenith. But the amplitude of the arch, between Dunnose and Arbury Hill, as derived from Capella, is 1° 36′ 20″·02, while those derived from the other two stars, are 1° 36′ 19.″42, and 1° 36′ 19″·94: a coincidence which proves that the instrument could not possibly have any perceptible deviation from the plane of the meridian at either station. Other reasons for coming to the same conclusion will appear, on attending to the precautions in adjusting by double azimuths, &c. as described in the Phil. Transactions.

or to its use in any way.

The correct position of the sector in *all* respects is further proved from this: that the observations, however distant in point of time, when the proper corrections for aberration, nutation, &c. are applied to them, reduce always very nearly to the same mean place.

There *cannot* be an error even of *half a second* at either Arbury or

Hence, it must be obvious, that no error could arise, as Don Rodriguez suspects, from the *instrument*, whether in "vertical position, construction, or some accidental derangement." I shall now advance still farther, and prove *that there is* NO ERROR *in fact*. For if there were any error in the zenith distances at Arbury Hill, it would at once be detected on comparison with the observations at Blenheim. Now, the distance between the parallels of latitude of Blenheim and Arbury,

139,822

139,822 feet, furnished by the survey, gives for the corresponding celestial arch, 22′ 59·″33, while the observations of γ Draconis at Blenheim, compared with the observations upon the same star at Arbury Hill, give 22′ 59·″6. So that there *cannot possibly* be an error of half a second at Arbury Hill, unless the observations, for five successive years at Blenheim, were all wrong: and Blenheim observatory, be it recollected, has been long celebrated for the excellency of its instruments; and is selected even by *Svanberg* for the accuracy of the observations there made.—So, again, with regard to the Dunnose station, the latitude of Portsmouth observatory, as inferred from the said station, and the data in the Trigonometrical Survey, is 50° 48′ 2·″05; while the Requisite Tables, the edition of 1781, give it 50° 48′ 3·″. So that the observations at Dunnose *cannot possibly* err half a second, unless there was an error made by Witchell and Bayley, in determining the latitude of Portsmouth observatory, with an admirable mural quadrant, by Bird. These two deductions, then, completely exclude sensible error at Dunnose and Arbury Hill: and these inferences, it is evident, might as easily have been made by Don Rodriguez as by me.

Dunnose, unless there be errors of the same, or greater magnitude, in the observatories at Blenheim and Portsmouth.

This gentleman may find still farther confirmation of the truth of the whole survey, if he will examine the operations by which the meridian of Dunnose is extended to Burleigh Moor, and those for carrying on a new meridian from Black Down to Delamere Forest. These, it is true, are not to be found (for what reason I cannot say) in the Philosophical Transactions. But they may be seen in the third volume of the Trigonometrical Survey, published in 1811, by order of the Board of Ordnance; a volume with which some of Don R.'s friends in England are doubtless acquainted.

As a last corroboration of the whole portion from Dunnose to Clifton, amounting to 2° 50′ 23″38; let me add, that when compared with the meridional arch of 3° 7′ 1″ at Peru, by means of the valuable theorem, investigated by Professor Playfair, (Edinburgh Transac. vol. v. pp. 8, 9) for the comparison of *large arcs*; it produces $\frac{1}{332\cdot68}$ for the resulting *compression*. While Svanberg (pa. 192, "*Exposition*") gives $\frac{1}{331\cdot448}$ for the compression, as deducible from a comparison of *his* measure with that at Peru.

Confirmation of the whole arch, furnished by Professor Playfair's formula, for the comparison of large arches; the only *fair* mean of inferring the ratio of the earth's axes, from terrestrial measures.

Thus, we have confirmation upon confirmation, of the correctness of Colonel Mudge's operations, both general and particular; and of the extreme rashness with which Don Rodriguez has affirmed, that "*it is very* evident that the zenith distances of stars taken at Arbury Hill are affected by *some considerable error.*" The matter in question might, as you will perceive, have been settled in narrower compass; but the celebrity of the institution under whose auspices the Don's animadversions are circulated, seemed, in some measure, to call for a tolerably full reply to his paper.

For the reply here presented, the public must consider me alone as responsible: and I trust that when the two papers have been compared, I shall not be thought to speak incompatibly with the courtesy due to a foreigner, or the respect due to a brother mathematician, when I say that Don Rodriguez has *completely failed* to establish the point, respecting which he ought to have felt *certain* before he commenced his strictures.

OLINTHUS GREGORY.

Royal Military Academy,
Woolwich, March 5th, 1813.

IV.

On the Existence of combined Water in muriatic acid Gas. By J. MURRAY, *Lecturer on Chemistry, &c. Edinburgh.*

To Mr. Nicholson.

Edinburgh, March 3, 1813.

SIR,

Late experiment of Sir H. Davy.

IN your Journal for January, an account is given of an experiment performed by Sir Humphry Davy in the College Laboratory of Edinburgh, in relation to the question on the existence of combined water in muriatic acid gas. I had found that the salt formed by the combination of this gas with ammonia, affords water when it is exposed to heat; and this water, I inferred, is derived from the acid. Sir H. Davy supposed it to be water which the salt had absorbed from the air; and he and his brother affirmed, that when the air is excluded, none is obtained. I resumed the investigation, and found that the salt

Muriate of ammonia does not absorb water from the air.

salt absorbs no water from the air, and that it affords water when heated, though the air has been excluded. The same results were obtained by Drs. Bostock and Traill. It remained, therefore, for Sir Humphry either to shew that they were not correct, or to establish, by farther evidence, his former statement. With this view the experiment, above alluded to, has been performed. About 90 cubic inches of muriatic acid gas were combined with the requisite quantity of ammoniacal gas, in an exhausted retort of the capacity of 20 cubic inches, and the salt formed having been heated in the same retort, closed at its extremity by a stop-cock, water was obtained from it in small quantity, "a dew just perceptible lining the cold neck." On this experiment I have now to offer a few observations, and I have to state the result of another since performed.

The retort in Sir H. Davy's experiment was too large.

When the experiment was made, I was informed by Dr. Hope of the result, and of the manner in which it had been executed. I stated to him in what respect it appeared to me objectionable, independent of the unfavourable circumstances inseparable from the mode of heating the salt in a close vessel; the large size of the retort rendering it difficult to apply the heat equally, so as to expel the water from one part without its condensing in another, allowing, too, a larger portion of any vapour disengaged to remain in the elastic form while the heat was kept high, and equally permitting its condensation when the heat diminished over an extensive surface, encrusted with a substance by which it would be absorbed, the unequal application of the heat producing a similar volatilization from one part, and condensation in another, the confinement of the heated elastic fluid operating by its pressure in resisting the separation of the water from the salt, and by its temperature counteracting the local condensation of the portion evaporated, and lastly, the encrustation of salt which had been allowed to remain at the curvature and upper part of the neck of the retort, where, in such an experiment, the condensation of moisture chiefly takes place, were all unfavourable to the result. If the experiment had been one in which a considerable quantity of water was to be looked for, these circumstances might have been of less importance. But this not being the case, it was more necessary to attend to their influence, and every arrange-

And the confinement prevented the separation and condensation of the water.

ment with regard to the experiment, ought to have been rendered favourable to the result, instead of being truly the reverse.

Fit conditions: a smaller vessel, heat equally diffused, no sublimed salt at place of condens. nor pressure, nor vacuum.

The principal circumstances which I conceived required to be attended to, were, to employ a much smaller vessel, to raise it through its whole capacity to an equal heat, to have the part of the apparatus in which the water is to be condensed free from salt, and to avoid, as far as practicable, the operation, either of pressure, or of a partial vacuum. It was nearly in this manner, that the experiment was performed by Dr. Bostock and Dr. Traill, and hence their successful result, while Sir Humphry, from not attending to these circumstances, was less successful, though performing it on a much larger scale. Dr. Hope, anxious to ascertain the matter of fact, readily agreed to repeat the experiment with these variations; Lord Webb Seymour and Mr. Ellis were present, and I have his permission to communicate the result.

Repetition of exp. before eminent men

Mur. of am. from dried gases was exposed to equal heat in a small retort.

Ammoniacal gas, previously exposed for two days to dry potash, and muriatic acid gas which had been exposed to dry muriate of lime for 24 hours, were combined in a dry exhausted flask, of the capacity of 3·8 cubic inches. About 90 cubic inches of the acid gas were employed, and the flask remained at the end filled with ammoniacal gas. The stop-cock being removed without exposing the salt to the air, a glass tube of four-tenths of an inch in diameter, previously fitted by grinding to the neck of the flask, was inserted, its open extremity dipping in quicksilver, and the flask being surrounded with sand in an iron box, was placed horizontally on a chafing dish, and fuel gradually introduced, so that the heat applied was slowly raised. In a short time moisture appeared in the tube, at a little distance from its insertion into the flask; this increased, proceeding to a greater extent along the tube, and condensing in globules perfectly distinct, which, at different periods of the experiment, covered the inner surface for a length of three, four, or six inches; and a small quantity collected at the under part, which, with a very slight inclination of the tube, moved slowly onward. At length the salt sublimed, and condensed in the tube close to the flask. The quantity of water, Dr. Hope was satisfied, appeared considerably larger than in Sir Humphry's experiment. The same quantities

Water was obtained, viz. about three-fourths of a grain;

tities of gases had been employed as in that experiment, and I need scarcely say, that every precaution had been taken to exclude every source of fallacy. Some of the salt having reached near to that place of the tube where the dew was condensed, part of the moisture seemed to have been resumed by it during the cooling of the apparatus, and prevented Dr. Hope from ascertaining with precision the quantity of the fluid. To obtain an estimate of it, he next day put a little water into another flask having a similar tube, previously weighed, fitted to it by grinding, and applied heat to the flask till the inside of a portion of the tube was covered with dew, and a drop of water collected in the bottom, as in the preceding experiment. The quantity of humidity, thus condensed, weighed one grain, and in appearance so far exceeded that observed in the tube in the experiment of the preceding day, as to lead to the conclusion, that the latter could not be estimated at more than two-thirds of a grain.

which exceeds that obtained by Sir H. D. and establishes the author's statements, &c.

Such is the result of these experiments intended to be decisive of the question with regard to the state of the fact, whether, when this salt is heated in close vessels, any water is obtained from it or not. Messrs. Davys affirmed, in the most explicit terms, that there is none; Sir H. Davy "did not observe the slightest traces of moisture in making the experiment on a larger scale in exhausted vessels." And Mr. J. Davy found, that "no water was produced—not even the slightest trace appeared." I affirmed, that though this mode of conducting the experiment is unfavourable to the result, and is not at all calculated to afford information with regard to the real quantity which the salt yields, still a sensible portion of water is obtained. It is now established, that my statement is correct, that of my opponents the reverse. In the experiment, as performed by Sir Humphry himself, a sensible portion of water appeared, and when the obvious sources of fallacy attending that experiment have been avoided, a larger quantity has been obtained.

Remarks. The vapour in Sir H. D.'s experiment did not come from extraneous sources.

To obviate the conclusion which might be drawn from this result, Mr. J. Davy endeavours to show, that the quantity obtained in his brother's experiment might be derived from extraneous sources, from vapour in the gases, or moisture from the mercury. This it is scarcely necessary to discuss. Dr. Henry, he remarks, found that ammonia obstinately retains aqueous vapour, yet Dr. Henry states, that ammonia may be so far desiccated by exposure to potash, " as to shew no traces of condensed moisture when exposed to a cold of 0° of Fahrenheit," and this precaution of exposing the ammonia to heat had been observed both in Sir Humphry's and in Dr. Hope's experiment. His brother, he adds, has proved, that a minute portion of solution of muriatic acid in water may be obtained by intensely cooling the gas. Dr. Henry, however, found, that muriatic acid gas, when freed from visible moisture, which it is completely by exposure to muriate of lime, (a precaution observed in the above experiments) deposits no water even when cooled to 20 below 0° of Fahrenheit, and Gay Lussac not only obtained the same result, but farther found no indication of moisture from the action of fluo-boric gas, which is its most delicate test. And, even according to Sir Humphry's statement, the quantity of liquid deposited from 200 cubical inches at 75°, cooled to 10 below 0, is not equal to $\frac{1}{18}$ of a grain, and only about half the weight of this is water. If any such water, therefore, is taken up by the gas at 50°, and retained by it after exposure to muriate of lime, of which there is no proof, but the reverse, it may amount, in 90 cubic inches, to $\frac{1}{75}$ or $\frac{1}{80}$ of a grain. Lastly, the mercury had been strained through warm linen, and was perfectly dry. The gases, therefore, having been submitted carefully to processes which are known to render them free from all moisture, being transmitted through dry mercury, and combined in an exhausted vessel, so that the mercury never came into contact with the salt, there is not the slightest reason to suppose a communication of water from any extraneous source. It is an obvious reflection, too, that if this salt is otherwise entirely free from water, as the new hypothesis assumes, were a minute portion communicated to it, it must be retained, in conformity to the law which universally regulates the combination of water with saline substances, by a very powerful attraction, so that it could not be expelled, and rendered sensible in such an experiment. And lastly, such causes are assigned by Mr. J. Davy only as " tending to account for the very minute quantity of water obtained" in his brother's experiment. They are, of course, still less adequate to account for the larger quantity in Dr. Hope's experiment; and are utterly incapable of accounting for the much larger quantity admitted by them to be obtained when the salt is heated in com-

If the salt were greedy of water, it would retain it, &c.

communication with the atmosphere, and which, it will be shewn, is derived from the salt, and not from the air.

Mr. J. Davy farther contrasts the small quantity of water obtained from the muriate of ammonia in his brother's experiment with the quantity which, according to the common doctrine, it contains; this latter quantity, he seems to imagine, ought to be procured; and, since it is not, he concludes that that doctrine cannot be maintained.

This kind of exp. is not calculated to show the quantity.

Any discussion with regard to the *quantity* of water obtained by heating the salt in a close vessel, is probably superfluous. That kind of experiment I never considered as one calculated to afford a proper indication of the real quantity which the salt yields. I repeated it merely because Messrs. Davys affirmed, that there is no appearance of water whatever. That assertion is now proved to be incorrect, which is all that the repetition of the experiment was designed to establish, and the original mode of conducting it I consider as the one which gives the true result.

Elucidation from various facts.

It may be remarked, however, to obviate any difficulty from this point, even with regard to the quantity obtained in the more favourable mode of conducting the experiment, that the combination of muriatic acid gas with ammonia, was not regarded as adapted to determine the proportion of combined water in the acid gas; for, of all the combinations of this acid, it is the one in which there is the greatest difficulty in separating the water. Acids, in combining with salifiable bases, retain the whole, or the greater part of their combined water, especially when these bases have also an attraction to water. To expel this from the compound salt to any extent, a heat, equal or superior to ignition, is in general required; and, by the most intense heat, it does not appear, that the whole quantity is expelled. Berthollet has shown, that after exposure to the violent heat of a forge, salts retain water, so that when again exposed to heat in mixture with iron filings, they afford hydrogen gas; and this is the case even with those which appear to have little attraction to water, as sulphate of barytes. Where the salt, therefore, is volatile, such as muriate of ammonia, the expulsion of its water must be imperfectly attained. The degree to which the heat may be raised is not great, and, in raising it, it must operate nearly with as much force on the real salt, as on the water combined with it, and their mutual affinity must retain them in union till both are sublimed together. If other salts which are fixed, and which have a less strong attraction to water, yield it only at a high temperature, and then imperfectly, it is absurd to imagine, that muriate of ammonia should yield it at a much lower temperature, and yield it entirely. The experiment, therefore, was designed rather to prove the *existence* of combined water in muriatic acid gas, and though the quantity obtained may not be the whole quantity which, from other facts, there is reason to conclude, exists in the acid gas, it establishes this as much as if a larger quantity were obtained. The production of any water is incompatible with Sir Humphry's hypothesis, and, therefore, refutes it; it is conformable to the opposite doctrine, and becomes, therefore, a proof of its truth; and for the quantity being less than that from other saline combinations of the acid, an adequate cause can be assigned. The actual result, indeed, is precisely that which is to be expected, a sensible portion of water more considerable as the experiment is performed in a manner more favourable to its disengagement, but inferior to what is obtained from other combinations of the acid, from which it is obvious, *a priori*, that the water must be more easily expelled.

It proves the existence of water, and establishes the doctrine.

So far I have restricted my observations chiefly to the result of the experiment of heating the salt in close vessels. A point not less important, which remained for determination, is that relating to the result when it is heated in open vessels, and to the supposed fallacy connected with this in the absorption of water from the air.

Whether the salt absorbs water from the air, as *supposed* by Sir H. Davy.

I had found that, in this mode of conducting the experiment, a very sensible quantity of water was obtained; and this was not denied, but explicitly admitted, by my opponents. Mr. J. Davy, who had heated the salt in close vessels, without obtaining water, found, that when he "followed Mr. Murray's example, and collected the salt in the atmosphere, and introduced it into another retort, on heat being applied, water, in no inconsiderable quantity, was evolved, as he described." But to account for this, without admitting the conclusion subversive of his hypothesis, Sir Humphry Davy advanced the supposition, that the salt absorbs water from the air during its transference from the one vessel to the other, and that this is the source of the water which it yields.

This supposition was not warranted by the facts;

A supposition so directly at variance with the known properties of this salt, required very ample proof, yet none was given of it, farther than the assertion of the salt not yielding water when heated in a close vessel, while it affords it when heated in an open vessel, this result being stated as affording "a demonstration, that the water liberated in Mr. Murray's experiment, was not derived from the muriatic gas, but from the atmosphere." It affords, I remarked, (Journal, vol. 32, p. 187,) no proof, since, admitting even the statement with regard to it to be correct, it might equally arise, since it is proved, that the salt yields water when it is heated without having been exposed to the air.

and might have been ascertained by direct experiment, but was not.

I had proposed the obvious experiment by which the fact, with regard to this supposed absorption of water, may be unequivocally ascertained—that of forming the salt without exposure to the air, and then ascertaining if, under such exposure, it gains weight, which it must do if it absorbs water. The mode of conducting the experiment, and the results, have been already minutely detailed (Journal, vol. XXXII, p. 191.) These results, proving that no water is absorbed, Messrs. [illegible]vys have not attempted to controvert, but have rather thought proper to avoid repeating the experiment, though it had been urged against them, and is obviously decisive of the question—for what reason I shall not conjecture.

Repetition of the experiment with Dr. Hope: Lord Webb Seymour and Mr. Ellis being present. A vessel with wide apertures at each end was taken;

The importance of the fact with regard to this supposed absorption is such, both from the supposition having been introduced to account for the production of water from the salt, and from its having led, in consequence of that, to a form of experiment which has rendered the investigation more difficult and more liable to error, that I was desirous the experiment should again be performed with every precaution. Lord Webb Seymour and Mr. Ellis were present, and the principal steps of the experiment were executed by Dr. Hope. A vessel was selected, the interior of which might admit of a free exposure to the air—it was pear-shaped, having a wide orifice at each extremity, the one, one inch and a half in diameter, the other, one inch, its whole internal surface being equal to about 40 square inches. The orifices were closed with corks rendered air-tight air-tight by cement, a stop-cock being inserted in one of them for the introduction of the gases.

in which the dried mur. ac. and ammon. gases were combined. Careful examination of the salt by the balance, during full exposure to the air, showed no increase, but a loss of weight.

The vessel having been exhausted, about 27 cubic inches of muriatic acid gas, which had been exposed for two days to dry muriate of lime, were combined in it with the requisite quantity of ammoniacal gas, which had been exposed for the same time to dry potash; and an excess of ammonia was allowed to remain at the end of the combination. The corks, with their cement, were removed, and clean corks, previously fitted, were instantly inserted. The vessel was filled with atmospheric air, by opening one of the orifices, and introducing a tube attached to a caoutchouc bottle, the sides of which being pressed together, and then allowed to dilate, drew out the ammoniacal gas: and to secure the change being complete, both corks were removed for a second or two. The apparatus was then placed in a balance, which, loaded with it, turned very sensibly with much less than $\frac{1}{20}$ of a grain. The balance being accurately adjusted, the corks were removed from the orifices, and placed beneath the vessel, and the progress of the experiment was observed. At the end of five minutes there was no perceptible change, of ten minutes no change, at fifteen minutes there was, if any thing, a loss of weight on the side of the salt, at twenty minutes this loss was apparent, and amounted to about $\frac{1}{20}$ of a grain, at twenty-five and at thirty minutes it remained the same. Though from the form of the vessel, and the size of the apertures, the air had the freest access to the salt which encrusted the interior; yet, to leave no doubt, the internal air was changed repeatedly by means of the caoutchouc bottle. At forty minutes there was again the appearance of loss of weight in the salt, at fifty minutes this amounted to something less than $\frac{1}{20}$ of a grain, in addition to the former loss. The air within the vessel was again repeatedly changed, both by means of the caoutchouc bottle, and by propelling the external air through it by the motion of the hand, and by the bottle, held at a distance and slowly compressed; but for half an hour longer there was no perceptible variation of weight*.

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* In a preliminary experiment which I had performed, and in which the salt was freely exposed to the air for three days, the loss of weight was

This experiment was performed in the same apartment in which my former experiments had been executed, and the air was at the same temperature of 60°. It is perfectly decisive in proving, that the salt absorbs no water from air in a common state of dryness and temperature.

A portion of this muriate of ammonia, being exposed to heat, gave out water.

As much of the salt was collected as could be removed from the vessel; it weighed 23·5 grains. It was introduced into a small retort connected with a small globular receiver, and the body of the retort being in part surrounded with sand, heat was applied by a lamp. A little of the salt suddenly rose in vapour into the neck of the retort. Afterwards moisture condensed beyond the salt where the neck was kept cool; the heat was slowly raised until the salt was sublimed into the top and beginning of the neck of the retort. The sand bath was then removed, a chaffing dish was applied, and the heat continued for half an hour. In the course of the experiment, the moisture increased, and extended over about one inch and a half of the upper side of the neck of the retort, where the cold was applied. The half of this space next to the bulb appeared quite wet, being covered with compressed globules of water of a considerable size, on the remaining part the globules were very minute.

More water inferred to have been separated in an exp. with higher heat.

I formerly related an experiment in which muriate of ammonia, after it had afforded a portion of water at a low heat, was sublimed through ignited charcoal, to ascertain if, by the higher temperature, and by the chemical affinities exerted by charcoal, an additional quantity might be abstracted. Portions of carbonic acid, and carburetted hydrogen gases, were accordingly obtained; and a quantity of water was condensed. This latter result led to the conclusion, that the high degree of heat had produced a more perfect separation of the water, and that, therefore, if such a temperature were applied to the salt alone, more water might be obtained from it than by an inferior heat, while any supposed source of fallacy from the presence of the charcoal, might be avoided.

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was apparent to a still greater extent than in the above experiment. Such a result, with regard to any other salt, would be ascribed to the abstraction of water by the agency of the air; and I see no reason why the same conclusion should not be drawn with regard to it. At the end of a week the salt remained perfectly dry.

A fact

Common sal-ammoniac has its first portion of water expelled in the manufactory, and does not afterwards attract more;

but it gives out another portion in an ignited tube.

Exp. of sublimation with the salt of direct combination.

A fact I had ascertained promised to afford a satisfactory mode of verifying this. The common sublimed muriate of ammonia, or sal-ammoniac, I had found, yields no water when exposed to a heat sufficient to sublime it. This is owing to its mode of preparation—it is first dried, then sublimed, and, during the sublimation, the upper part of the vessel is kept hot, to render the sublimed mass sufficiently dense, its orifice being also kept open, and hence all the water which can be driven off by this heat is expelled, and none is regained by exposure to the air (a decisive proof, if such were wanting, that this salt attracts no water from the atmosphere, since it is kept in the shops without any particular precaution. I exposed 100 grains of this salt in a retort to a heat sufficient to produce sublimation, but no moisture appeared during any part of the experiment. I then sublimed 100 grains of the same salt from the close end of a porcelain tube, placed across a furnace so as to be at a red heat. A very sensible quantity of moisture condensed in a glass tube, which was adapted to the porcelain one, appearing not only in globules, but at length running down the tube. This proved, that water may be separated from muriate of ammonia by a red heat, which is not expelled from it at a lower temperature. I then submitted to a similar experiment, the salt formed by the direct combination of its elements. Very little moisture appeared previous to its actual volatilization, but when this commenced, the condensation of water in sensible globules took place; they continued to accumulate, and the quantity appeared obviously greater than what, judging from former experiments, would have been obtained by a lower heat from the salt formed from the same quantity of muriatic acid gas.

Repetition in an ignited tube.

In another experiment, the salt formed in an exhausted retort was first heated until it ceased to afford water, and was afterwards sublimed through an ignited porcelain tube. Moisture was again obtained, though not in so large a quantity as when the charcoal had been placed in the tube. There is no just objection to the introduction of the agency of the charcoal, if care be taken to have it thoroughly calcined; and, as the supposed source of fallacy from the air affording water to the salt, is now proved to have no existence, there is no valid objection to the result which the experiment with the charcoal affords.

Inference. The salt does afford water, and did not acquire it from the atmos.

My preceding conclusions, I trust, are now sufficiently established, and it is unnecessary to enter on any recapitulation of the argument. Water has been obtained from this salt both when it is heated in close and in open vessels; and no source of fallacy exists, as was affirmed by Messrs. Davys, in any absorption of water from the atmosphere. They accounted for the production of water on that supposition, and it is now amply refuted.

Considerations relating to personal aspersion and censure.

I have only a single observation to make on Mr. J. Davy's concluding remarks in his last communication, that he has "no intention of answering personal aspersions, which are only injurious to the author when unjustly made." The necessity was imposed upon me by assertions which he had advanced of stating some circumstances connected with the manner in which he and his brother had conducted the controversy. I did so with reluctance, and only in so far as was necessary to my own vindication from a very intemperate attack. My observations conveyed censure, no doubt, but not aspersion; for they were founded on facts, and these were very explicitly stated, that Mr. J. Davy might, if he pleased, enter into any explanation with regard to them. This he has not done, and the facts, I believe, he is unable to controvert.

From a review of the manner in which the author's results were discussed, he suggests, that it might, with propriety, have been more modest and temperate.

In concluding this investigation, I cannot but contrast the assertions that were made, and the tone that was assumed, with the result that has been established. "At first view," said Mr. J. Davy, speaking of my experiment of obtaining water from muriate of ammonia, "the result appears improbable, and opposed by several facts; and, in a very short time, I was convinced by experiments that it was incorrect." Again, "Mr. Davy, my brother, informed me, that he had not observed the slightest traces of moisture in making the experiment on a large scale in exhausted vessels; and assured me, that I should not, was not the salt exposed to the atmosphere." In repeating the experiment accordingly, no water was produced "agreeably to my brother's result, not even the slightest traces appeared." Mr. Murray's errour," he adds, "appears to have arisen partly from too great confidence placed in the accuracy of his experiment, and partly from overlooking, that a light powdery substance, like muriate of ammonia, independent of its chemical attraction, absorbs water hygrometrically. Mr. Davy has informed me, that this is the case, and that muriate of ammonia so made, absorbs so much, that it even deliquesces." And lastly, " Mr. Murray's confidence in his result, which is opposed by several facts relative to muriate of ammonia, is to me more surprising than the result itself." When assertions and conclusions have been advanced in this unqualified manner, which the result of investigation proves to be wholly incorrect, it is but justice to recall them for a moment to notice; and when such a style of controversy has been indulged in, it is not uncandid to suggest the reflection, how much more becoming would have been a more modest and temperate tone. I shall refrain from farther animadversion on a topic ungrateful in itself, and too unimportant to claim any protracted discussion.

With the highest respect,
I remain
Your most obedient Servant,
J. MURRAY.

V.

On the Explosive Compound of Chlorine and Azote.

(Concluded from p. 190.)

To Mr. Nicholson.

SIR,

IN conformity with our promise made to you in our former communication, we resume the account of our experiments with the explosive compound.

Globule of the compound exposed under water to voltaism.

A globule of the compound was placed under water, between the ends of two platina wires, coated with glass excepting the points; one of these wires communicated with the positive, and the other with the negative end of a voltaic trough, containing 50 pairs of six-inch plates, excited by weak muriatic acid. The globule appeared to be little, if at all, affected by the current of the electric fluid, of which, we are inclined to believe, it is not a conductor: small bubbles of gas rose from it occa-

No effect under these circumstances.

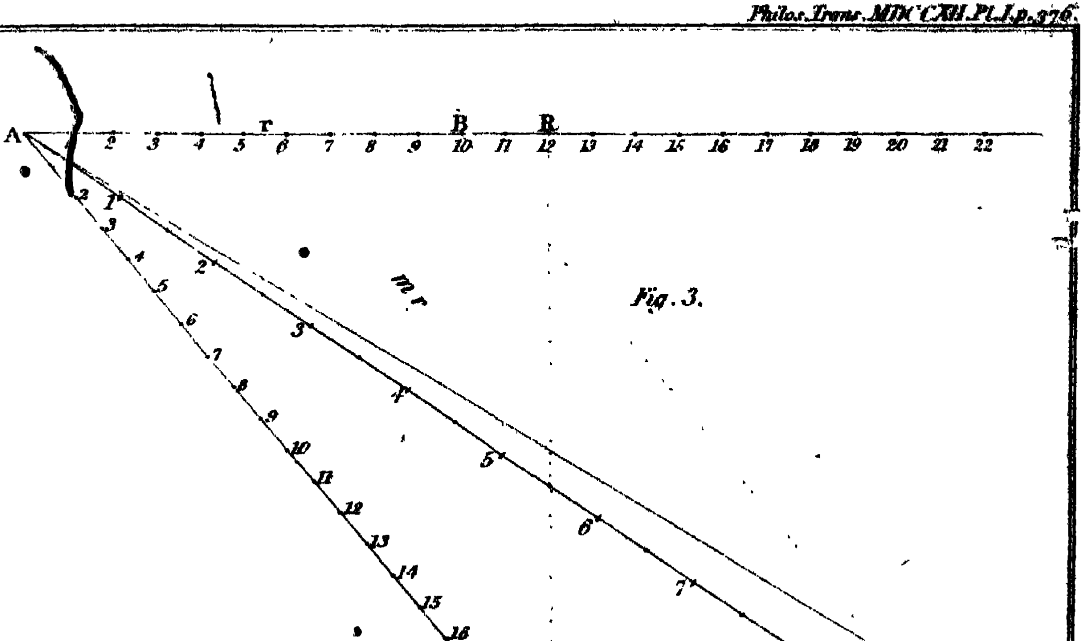

Fig. 3.

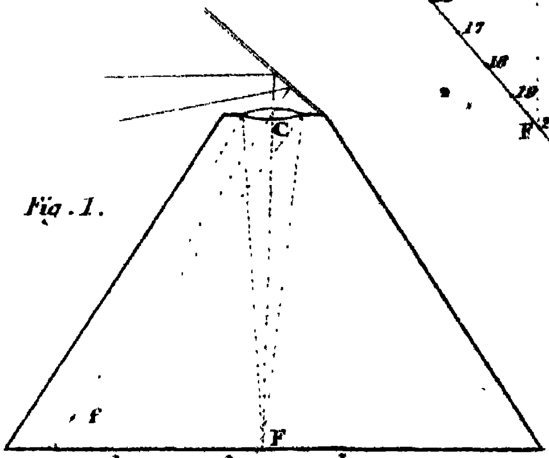

Fig. 1.

Common Camera obscura

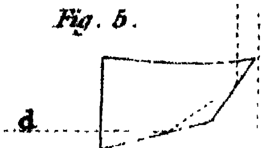

Fig. 5.

Periscopic prism of Camera lucida

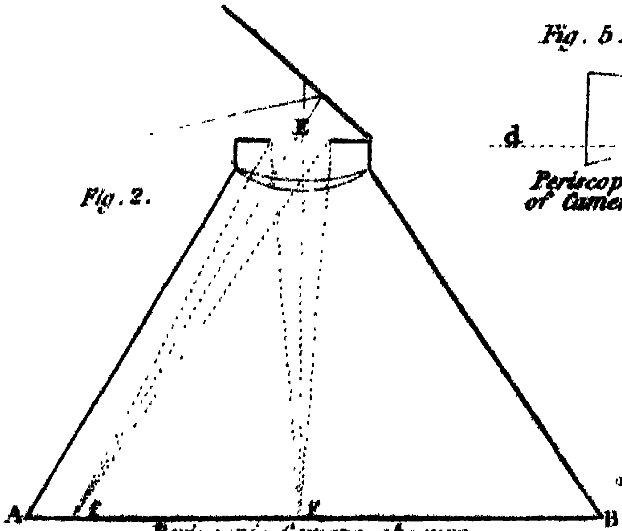

Fig. 2.

Periscopic Camera obscura

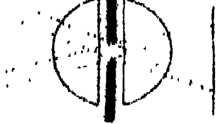

Fig. 4.

Periscopic microscope enlarged

H. Cooper sculp.

occasionally, but as the water with which it was in contact was undergoing rapid decomposition, it is not unlikely that these bubbles were not caused directly by the electric fluid, but by the hydrogen or oxigen liberated from the water, acting on the compound. This source of ambiguity would be removed, if the compound could be electrised without its being in contact with any fluid; but its extreme volatility presents an obstacle to such an arrangement, which we have not yet surmounted.

Phenomena produced by contact of the explosive compound, and various substances.

Having made a number of experiments for the purpose of ascertaining generally the phenomena resulting from the contact of various substances with the explosive compound, we have made out the following table, in the first column of which are stated the several substances employed, and in the second the apparent effects of them on the compound. It is proper to remark, that water was always present in these experiments, the general method of making them having been to place a globule of the compound in a small iron ladle filled with water, and to bring the substance, whose action on it was to be tried, into contact with it at the bottom; but in those cases in which it was desirable to have as little water present as possible, we have substituted for the ladle a little paper filter, containing the compound and water, and allowed nearly all the water to drop through, before we added the substance to the compound. This was our mode of operating with ether, alcohol, &c.

Table of the apparent Effects of certain Substances brought into Contact with the explosive Compound.

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the compound.	Effects observed.
Mercury	Slight effervescence, the metal slowly tarnished.
Copper	do. do.
Tin	None.
Zinc	None.
Sulphur	None.
Liquid sulphuretted hydrogen, or alcohol of sulphur	None.
Super Sulphuretted hydrogen formed by adding hydroguretted sulphuret of potash to muriatic acid. ..	Violent explosion.

Do.

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the compound.	Effects observed.
Do. become solid by keeping.	Union, but no explosion
Native sulphuret of antim.	None.
Red sulphuret of mercury.	None.
Phosphorus	Explosion extremely violent.
Phosphuret of lime	Violent explosion.
Phosphorus dissolved in liquid sulphuretted hyd.	Do.
Charcoal	None.
Jet	None.
Cannel coal	Adhesion, slight effervescence.
Residuum of the distillation of amber	Effervescence, film on the surface of the water.
Asphaltum	Union, rapid effervescence, ascent to the surface of the water, film left there.
Elastic bitumen	Union.
Elastic gum, or caoutchouc	Violent explosion.
Resinous matter found in Highgate Hill	None.
Common resin	Effervescence.
Shell Lac	None.
Copal	Union, effervescence, ascent to the surface of the water, film left there.
Sandarach	Adhesion.
Mastich	Same as with copal.
Euphorbium	Do. do.
Guiacum	Do. do.
Assafœtida	Do. do.
Opium	Slight effervescence, film on the water.
Burgundy pitch	Union, effervescence.
Balsam of Tolu	Do. do. rapid, film on the water.
Resin of ox bile	Slight effervescence.
Myrrh	Explosion.
Scammony	None.
Frankincense	Do.
Ammoniacum	Do.
Hepatic aloes	Do.
Alcoholic solution of resin	Rapid effervescence.
Do. of resin of lac	Do.

Cam-

Substances brought into contact with the compound.	Effects observed.	Effects of substances brought into contact with the explosive compound.
Camphor	Union in considerable quantity with the compound, which preserves its usual colour and appearance. When the camphoretted compound rises to the surface of the water, it covers it with a film of camphor, the explosive compound escaping from it. The camphoretted compound inflames without explosion by phosphorus and by essential oils. Sulphuric ether separates the camphor from it. It may be formed at the same time with the explosive compound, by introducing a bit of camphor into chlorine gas over solution of muriate of ammonia.	
Phosphurretted camphor	Explosion.	
Sulphuretted do.	Union.	
Wax	None.	
Spermaceti	None.	
Adipocire	None.	
Butter	None.	
Palm oil	Explosion.	
Do. saturated with chlorine, which made it white and semifluid	Union.	
Oil of mace	Do. rapid effervescence, film on the water.	
Ambergris	Explosion.	
Hogs' lard	None.	
Whale oil	Explosion, separation of carbon.	
Linseed oil	Explosion.	
Olive oil	Do. separation of carbon.	
Do. do. saturated with chlorine	Union.	
Do. do camphoretted	Violent explosion.	
Do. do. sulphuretted	Do.	
Do. do. thickened by boiling on oxide of mercury.	Effervescence, explosion.	

Do.

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the compound.	Effects observed.
Olive oil, by boiling on corrosive sublimate	Union.
Oil from soap by sulphuric acid	Union, effervescence, film on water.
Do. do. by nitric acid	Do. brisk do. film on water.
Oil of turpentine	Violent explosion.
Do. do. sat. with chlorine	Union; by the application of flame to it on the surface of water, it deflagrates, and leaves a resinous looking substance on the water.
Oil of tar.	Violent explosion.
Do. of amber	Do.
Do. of petroleum	Do.
Do. of Benzoin	Union, effervescence, remarkable change of colour to blood red.
Do. of orange peel	Violent explosion.
Naptha	Rapid effervescence, explosion.
Alcohol	None.
Sulphuric ether	None.
Nitric ether	None.
Phosphuretted ether	Violent effervescence.
Soap of potash	Rapid effervescence.
Do. of soda (curd soap) ..	Union, effervescence, film or water.
Do. of do. (Castille)	Do. Do. Do.
Do. of barytes (from nitrate)	Slow effervescence.
Do. of alumine (from sulphate)	Much effervescence.
Do of lime (from nitrate)	Slow effervescence.
Do. of strontia (from do.) ..	Do. Do.
Do. of magnesia (from sulphate	Do. Do.
Do. of silver (from nitrate)	Violent explosion, blue flame.
Do. of protoxide of mercury (from nitrate.)	Do. Do. white flame.
Do. of peroxide of mercury (from nitrate)	Do. Do.
Do. of copper (from nitrate.)	Do. Do.
Do of lead (from nitrate.)	Do. Do.
Do. of do. (litharge plaister.)	Do. Do.
Do. of tin (from muriate.)	Effervescence.
Do. of cobalt (from nitromuriate.)	Do. Do.

Substances brought into contact with the compound.	Effects observed.	Effects of substances brought into contact with the explosive compound.
Soap of platina (from nitro-muriate.)	Effervescence rapid, film on water.	
Do. of manganese (from sulphate.)	Violent explosion.	
Sugar	None.	
Manna	None.	
Gum Senegal	None.	
Starch	None.	
Indigo	None.	
Kino	None.	
Catechu or terra japonica.	None.	
Extract of logwood.	None.	
Benzoic acid.	None.	
Albumen (dried.)	None.	
Prussiate of iron.	None.	
Triple Prussiate of potash in crystals.	None.	
Sulphuric acid.	None.	
Nitric acid	None.	
Muriatic acid	None.	
Phosphorous acid.	None.	
Fused potash pure.	Explosion, owing to the heat produced by the potash dissolving in a small quantity of water.	
Solution of do.	Effervescence.	
Solution of pure ammonia.	Violent explosion.	
Do. diluted with its bulk of water.	Rapid effervescence.	
Lime	Effervescence.	
Carbonate of lime.	Do.	
Red oxide of lead.	Do.	
Nitrate of silver.	Muriate of silver formed, the compound disappeared.	
Hidrogen gas.	The compound disappeared immediately, the volume of the gas increased.	
Super-carburetted hidrogen, or olefiant gas.	The compound disappeared immediately.	
Phosphuretted hidrogen gas.	Do. with explosion.	
Sulphuretted do. do.	Do. opacity in the gas, precipitation of sulphur.	
Arseniuretted do. do.	Do. precipitation of arsenic.	
Oxigen gas.	Do.	

Effects of substances brought into contact with the explosive compound.

Substances brought into contact with the explosive compound.	Effects observed.
Azotic gas.	Do. precipitation of arsenic.
Atmospheric air.	Do.
Nitrous gas.	Do. violent explosion, blue flame.

Remark upon the table, and the general effects.

In performing the experiments, the results of which are stated in the preceding table, our intention was not to investigate minutely the changes produced in the substances made to act on each other, but to acquire a knowledge of the principal and most obvious effects of the explosive compound on a variety of bodies. This, we trust that we have accomplished; and, in so doing, have discovered some curious and interesting facts; amongst which the following appear to be most deserving of notice.

Combustibles act most strongly on the compound.

1st. The class of bodies which act on the explosive compound with the most energy, are those which are termed combustible bodies. There are, however, some few exceptions to this remark, instanced in the want of action of ether and of alcohol.

The effects appear to arise from dense chlorine.

2d. That there is a considerable analogy between the action of the explosive compound, and that of the chlorine and euchlorine, separated in a condensed state by strong sulphuric acid, from the salt, known by the name of the oxymuriate of potash; which, like the explosive compound, inflames volatile oils, caoutchouc, phosphorus ammonia, &c. And that most of the effects of the explosive compound are attributable to chlorine in a condensed state, and in weak chemical union.

Camphor, &c. unite without decomposition.

3d. That there are some combustible bodies which will unite without decomposition, with the explosive compound, of which camphor is a remarkable instance.

No action with saturated bodies.

4th. That when a combustible body is previously saturated with chlorine, its action on the explosive compound is either annihilated or much weakened.

Animal substances have less action than vegetable.

5th. That animal substances in general appear to act with less energy on the explosive compound, than their analogous vegetable substances. The want of action of adepocire, of spermaceti, of butter, and of lard, are striking proofs of the truth of this assertion.

Soaps, by double de-

6th. That there is a remarkable difference in the actions on the compound of the several soaps formed by double decomposition

position of saline solutions, and solution of soap; as it appears that the earthy soaps do not explode with it, and that of the metallic soaps, those prepared from nitric salts explode, while those prepared from muriatic salts do not.

composition, do not explode.

The experiments may, in some cases, vary on repetition.

Of the numerous experiments, of which a statement is given in the preceding table, we will not pledge ourselves that all are equally accurate; we have taken considerable pains that they should be so, but their number has hitherto prevented us from repeating the greater part of them. The repetition of some of them has convinced us, that very minute circumstances will sometimes cause the results to vary. Should we hereafter find it necessary to correct any involuntary inaccuracies in our statement, we shall do it with confidence in the indulgence of the readers of your Journal.

Apparatus for exposing the compound to gas in a closed vessel.

As it may be expected, that we should describe our mode of bringing the explosive compound into contact with confined portions of the gases, we have represented our apparatus for this purpose in the sketch, plate VI. fig. 3, to which the present explanation will apply. (*a*) A small capsule of bone or ivory, having a small hole in its centre—this capsule is suspended by a string, passing air-tight through the top of a glass receiver (*e*) between a collar of wetted leather, which serves to secure in its place the stop-cock (*b*)—this stop-cock has a connecting screw, to which the stop-cock (*c*) of the bladder (*d*) can at any time be attached. (*f*) is a water bath.

and method of using it.

When this apparatus is to be used, the capsule (*a*) is to be drawn down, so as to bring it on the outside of the glass receiver: the bladder with its stop-cock is to be unscrewed and filled with the gas intended to be used; the stop-cock (*b*) is to be opened, and by the action of the mouth applied to it the water is to be drawn up so as to fill the receiver. The cock (*b*) is then to be shut, and the cock (*c*) with the bladder of gas screwed on. A small piece of blotting paper is then to be laid on the hole in the capsule, on which the globule of the explosive compound is to be placed. The capsule is then to be placed again under the receiver, and by means of the string on the outside, drawn up into the receiver full of water, to such a height as may be thought necessary; after which, the two stop-cocks are to be opened, which will admit the air from the bladder into the receiver; the water in which will all descend as the air enters, excepting what is retained in the capsule, and which covers the globule of the compound; but as this small quantity very quickly filters through the blotting paper, and falls in drops through the hole in the capsule, the compound is left exposed to the gas, and the effects of this exposure immediately appear.

When the compound is kept with water in a sealed tube, it becomes dissolved in process of time, unless the quantity of water be inconsiderable.

In our former communication we mentioned that the compound may be preserved for any length of time, in small tubes hermetically sealed, provided that the quantity of water, or of air, included with it, did not exceed seventeen times its bulk; we have since found that this is strictly true, only when the quantity of water in the tube is very inconsiderable compared to that of the air included; for that when the tube is nearly filled with water, the compound, after some months, disappears and is dissolved in it.

Remarks on sources of errour in the former analysis.

In the same communication we described an analysis of the compound, remarking, at the same time, that not having repeated it, we could not place any confidence in its results, and that our principal object in giving an account of it was, to show an easy and practicable mode of analysing the compound. In the interval, since that was published, we have paid particular attention to that analysis, and have found that there were two very material sources of error in it; the first was owing to the imperfect means which we then possessed of obtaining two globules of equal weights; and the second, to a circumstance of which we were not then aware (but which our subsequent experiments have proved to have a considerable influence) viz. that the quantity of water with which the explosive compound was in contact when it was decomposed by potash, was much too large, and occasioned less azotic gas to be given out, than would otherwise have been collected. To obviate these two sources of error has been the object of our recent labours, and we have fortunately succeeded in removing both.

The globules of the compound were unequal, and too much water was present.

Remedy. The exact measure of the compound was ascertained by a capillary syringe,

The mode by which we have succeeded in always operating with known weights of the compound, is by using a glass syringe, of the form represented in fig. 4. pl. VI. the lower part of which terminates in a tube of small bore; such as is used for thermometers. This tube is graduated on the outside into inches and decimal parts, and when the point is placed in a globule of the explosive compound under water, and the piston raised, the compound may be drawn up in an uninterrupted line to any mark on the stem that may be desired: the inch measure of the stem of our syringe holds exactly 5·3 grains of pure mercury; consequently it must hold ·625 of a grain of the explosive compound, the specific gravity of mercury being 13·568, and that of the explosive compound, according to our experiments, being 1·6.

The mode by which we obviated the errour arising from having much water present, was to decompose the compound over mercury in the following manner:

and the quantity of water taken very small, over mercury.

A small stoppered phial was converted to an air receiver, by having its bottom cut off—it was then sunk up to its neck in the mercury contained in a small mercurial trough, the stopper being first taken out. The capacity of the neck was then filled with a few drops of water, into which was introduced the ·625 of a grain of the explosive compound—the glass stopper was then put into its place, and the receiver, with its contents, raised on to the shelf of the trough. Some potash was then procured, which was free from carbonic acid, and had been deprived of any combustible matter, by having undergone igneous fusion; it was also free from any metallic oxide. Of this potash a concentrated aqueous solution was made, and this solution passed up into the receiver to the compound, the decomposition of which it occasioned. In performing this experiment, it is of importance not to pass up the fused potash in the solid state, as the heat which is occasioned by its solution in the small quantity of water which it meets with, instantly causes the compound to explode.

Solution of potash added,

The decomposition of the compound by liquid ammonia we effected exactly in the same manner, passing up the solution of ammonia instead of the solution of potash; the solution of pure ammonia must, however, be diluted with its own bulk of water, otherwise it will immediately occasion an explosion of the compound.

or of ammonia.

By these arrangements, we believe that we have removed every source of error; and, having repeated the analysis several times with the greatest care, and with scarcely any variation in the quantities of gases obtained, we are enabled to give those quantities with considerable confidence. This will be best done by stating the particulars of two of our experiments.

The analysis thus made is entitled to confidence.

Exp. 1. Decomposition by potash.

1st Exp. (Barometer 30·4, thermometer 55°) ·625 of a grain of the explosive compound was decomposed by solution of potash, in the manner just described; the quantity of gas obtained was ·25 of a cubic inch; phosphorus was sublimed in it; after which operation, and being again cooled, its volume was ·245 of a cubic inch; which being phosphuretted azotic gas, must be corrected for an increase of volume of $\frac{1}{40}$ by phosphorus in solution. This brings it to ·239, which, brought to the mean temperature and pressure, becomes ·2447 of a cubic inch, being the quantity of azotic gas derived from the compound.

Exp. 2. Decomposition by ammonia.

2d Exp. (Barometer 30·4, thermometer 55°) ·625 of a grain of the explosive compound was decomposed by a solution of pure ammonia, diluted with its bulk of water, in the manner before stated. The quantity of gas obtained was ·395 of a cubic inch: after subliming phosphorus in it, it was ·41, which, corrected for increase of volume by phosphorus in solution, for pressure above the mean, and for temperature below it, becomes ·4095 of a cubic inch, being the quantity of azotic gas derived both from the compound, and from the decomposition of the ammonia by the chlorine of the compound. Then to know how much is derived from the latter source only, we have only to deduct the quantity ascertained by the first experiment, from that ascertained by this experiment. This being done, the remainder is ·1648, which remainder, representing three times its volume of chlorine gas, gives ·4944 of a cubic inch, as the volume of chlorine gas contained in ·625 of a grain of the explosive compound.

The quantities of azotic and chlorine gases in the compound

The quantity of azotic gas in ·625 of a grain of the compound being ascertained by the first experiment, and that of the chlorine gas in the same weight of the compound being known by the second experiment, it is obvious, that if the experiments are accurate, and the compound consists of chlorine and azote only, then the weights of those two gases should exactly correspond with the weight of the compound, viz. ·625 of a grain.

But, according to the following calculation, this is not the case.

Weight

c. inches.	
Weight of ·2447 of azotic gas (Biot and Arrago)	·0735 of a grain,
Do. of ·4944 of chlorine gas (Gay Lussac and Thenard)	·3724 of do.
Total	·4459 of do.

Here, then, we have a deficiency of ·1791 of a grain, for which we must account either by concluding that our analysis is inaccurate, or that the explosive compound contains some other constituent part besides azote and chlorine.

are less than the whole weight.

But from having repeated our analysis several times, we are convinced, that it is free from errors of any consequence: we, therefore, conclude, that azote and chlorine are not the sole constituents of the explosive compound.

But as the analysis was accurate,

What other, then, does it contain? To answer this question we must first consider what others it can possibly contain, and we shall find that no others, excepting oxigen or hydrogen, can possibly enter into its composition, because, in the simplest cases of its formation, no other bodies are present than chlorine, azote, oxigen, and hydrogen.

the compound must have some other component,

Now, if it contained oxigen as a third substance, the results of the decomposition of the compound by ammonia, would be different from what we find them; for, in that process, the oxigen must either assume the gaseous form, which it does not; or, if it be supposed to form water with the hydrogen of the ammonia, then it must displace five times more azotic gas than chlorine would, because any given weight of oxigen combines with five times more hydrogen than the same weight of chlorine does. Instead, therefore, of collecting too little azotic gas, we should have had a very considerable excess.

which does not appear to be oxigen.

The supposition, therefore, that oxigen is the third substance contained in the explosive compound, is, in the highest degree, improbable, and inconsistent with the results of our experiments.

It must, therefore, be hydrogen which is the third substance.

but hydrogen

But in whatever proportion the hydrogen may exist in the compound, it must, by combining with a certain portion of the chlorine in that compound, neutralise the decomposing

Deduction, that the hydrogen, being in combination

action

with chlorine, amounted to a quantity stated.

action of that portion on ammonia. This portion of chlorine would not, therefore, be indicated in the action of the compound on ammonia by the separation of one-third its volume of azotic gas, it having combined with hydrogen at the expence of the compound, and not at the expence of the ammonia. In the before-mentioned analysis there is, therefore, a deficiency of a certain portion of hydrogen, and of a certain portion of chlorine; but, as the total deficiency of both is known, (being the difference between the weight of the compound analysed, ·625 of a grain, and that of the gases ascertained by the analysis, ·4459 of a grain) viz. ·1791 of a grain; and as the proportions in which chlorine combines with hydrogen are also known, (being equal volumes of the two gases, or by weight 1 hydrogen to 30·148 chlorine) it follows, that the volumes and weights to be added are,

c. in.		grain.
·23012	of chlorine gas weighing	·17335
·23012	of hydrogen gas, do.	·00575

Whence the component parts, as corrected, are ascertained;

With these additions to the analysis, the composition of ·625 of a grain of the explosive compound will be as follows:

c. in. at mean temp. and pres.		grains.
·7245	chlorine gas	·54575
·2447	azotic gas	·07350
·2301	hydrogen gas	·00575
1·1993	condensed in the compound $\frac{1}{777}$ their volume	·625

or otherwise by a different arrangement of the components.

Or its composition may be stated in a different form, upon the supposition that the elements arrange themselves in the following way; the hydrogen, with part of the chlorine, being in the state of muriatic acid.

c. in. at mean temp. and pres.		grains.
·4944	chlorine gas	·3724
·4602	muriatic acid gas	·1791
·2447	azotic gas	·0735
1·1993	condensed in the compound $\frac{1}{777}$ their volume	·625

An objection may be made to the above reasoning and conclusion, on the ground that we have not taken into consideration the possibility of the explosive substance being a compound of chlorine, azote, oxigen and hydrogen; and it may be said, that the arguments for the exclusion of oxigen from the compound, drawn from its action on ammonia, will lose all their force if it is considered as a quaternary combination instead of a ternary one; because the oxigen and hydrogen in the compound may be in the state of water; in which case neither of them would appear in the gaseous state by the action of ammonia, nor could the oxigen displace azote from that alkali.

An hypothesis may be made that oxigen and hydrogen may be present in the form of water.

In answer to such an objection we have to observe, that the supposition that the explosive substance is a quaternary compound of chlorine, azote, oxigen and hydrogen, being at present unsupported by experiment, we conceive that the following reasons will justify us for refusing to admit it.

But this quaternary compound is not probable.

1st. It is not consistent with the cautious principles of philosophical reasoning to admit four elements in a compound, so long as its properties and actions on other bodies are explicable by three.

Reasons.
1. The rule of philosophising.

2d. From the known affinities of the four elements abovementioned, and from the proportions in which they must exist in the explosive compound on the supposition under consideration, we infer that they must be combined in ·625 of a grain of the explosive substance, in the following manner:

2. That such a compound would have different properties.

grs.		
·372	chlorine	forming muriatic acid,
·011	hydrogen	
·773	azote	forming nitrous gas,
·083	oxigen.	
·076	oxigen	forming water.
·010	hydrogen	
·625		

In which case the characteristic properties of the compound would be those of muriatic acid and of nitrous gas, and not those of chlorine, which is contrary to the fact.

Should, however, it be proved, by satisfactory experiments, that the explosive substance contains oxigen, our statement of the

But if experiments should establish such a compound, the deduction must be changed.

the composition of that substance must be modified, by admitting, that the hydrogen in the compound is neutralized by oxigen instead of by chlorine.

The hydrogen is the link of union,

The hydrogen in the compound appears to be the link, connecting together the chlorine and azote, by its affinities for both, in the same manner as it does in the ternary compound with chlorine and carbon, formed when supercarburetted hydrogen gas and chlorine gas are mixed together.

and prevents the compound from being decomposed by water.

It is the hydrogen in it also that, in all probability, prevents it from being instantly decomposed by water, by weakening the attraction between the chlorine of the compound and the hydrogen of the water; so that it is not able to overcome that which unites the elements of the water.

Instead of Sir H. Davy's theory of chlorine, the earlier theory of ox mur. acid may probably be applicable.

It will be observed, that we have adopted the system of Sir H. Davy, with respect to the nature of what was formerly called oximuriatic gas. The several phenomena resulting from the action of the explosive compound on other bodies, however, are probably also capable of explanation on the old theory; and such of your readers as may wish to apply that theory to these phenomena, have only to consider them as resulting from the transfer of oxigen from the oximuriatic acid of the compound to the combustible body, forming oxigenised products, such as carbonic acid, oxides, &c. and to the separation of the muriatic acid from the compound, in consequence of this loss of oxigen.

Explanation.

On Sir H. Davy's system, these phenomena are considered as resulting from the attraction of the chlorine for combustible bodies, and most usually for hydrogen, which it takes either from a combustible body containing it, or from the water present; and, in these cases, the muriatic acid is formed from this union of chlorine with hydrogen; and oxygenised products are also formed whenever the hydrogen, which thus unites to the chlorine, is derived from the water.

We are, Sir,
Your most obedient, humble Servants,
R. PORRETT, Jun.
W. WILSON.
RUPERT KIRK.

London, 16th March, 1813.

VI.

Vindication of the Claims of the American Boy to extraordinary Talents and original Discovery. In a Letter from Mr. W. SAINT.

To Mr. Nicholson.

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

SIR,

IN reading your last number, I was struck with surprise (in common, it should seem, with most of your readers) to find that you had inserted a letter from the *Morning Chronicle*, which purported to give an account of the manner by which the *American boy* performs his calculations with such wonderderful celerity. Now I am persuaded, Sir, that, had you had sufficient leisure to examine into the merits of that letter, and into the claims of its author to the important discovery which he affects to have made, you would not have given publicity, (and, what is of still greater consequence, *your sanction*) to a statement so little calculated to effect the object of its author, which was "to reduce the child to what he really is—a very clever boy, but no prodigy."

Your insertion of this letter, after the very excellent account you gave of the boy in a former number, has tended to produce a belief in the minds of such of your readers as are unaccustomed to abstruse calculations, that what this child does may likewise be effected by *any other boy of good abilities*, and thus a prejudice may be excited against this youthful and astonishing calculator, which may prove equally injurious to his *own fame*, and to his father's *pecuniary interest*. I have, therefore, to request, Sir, that you will assist me in my efforts to vindicate the reputation of this extraordinary boy, by inserting in your *next* number, if convenient, the following remarks on the letter alluded to, in which I have endeavoured to show, that Mr. A. H. E. has not succeeded in discovering the method by which this boy performs his calculations with such surprising celerity.

In the application of M. *Rallier's* method to the extraction of the *cube* root, Mr. A. allows, that "the result is *ambiguous* where the number proposed terminates with an *even* digit, or with a 5;" he proceeds, however, to explain how the difficulty may

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may be removed with respect to the *even* digit, though I think I may safely challenge him *to produce a single instance of a child from* SIX to EIGHT *years of age*, who would be able to *comprehend* the method, much less to apply it with *facility* and *rapidity*. Be this as it may, it is confessed by Mr. A. that the case of numbers ending with 5 is one which "*can deceive*," and I accordingly expected to find that Mr. A. had given the boy various examples of this *ambiguous case*, and that he had uniformly found the boy incapable of answering such questions *correctly*, or that he had obtained from him *an acknowledgement* that such questions were beyond the reach of his powers to answer. Yet nothing of this kind is mentioned by Mr. A. who leaves us totally in the dark upon the very point which would have cleared up the difficulty. Are we to imagine, then, that Mr. A. though aware of the importance of putting such questions, for the purpose of ascertaining whether M. *Rallier's* method was employed or not, yet omitted to ask them? Or, if he did ask questions of this kind, and received *wrong* answers, (which must have been the case if the boy employed the *method alluded to*,) how is it that he has neglected to avail himself of the statement of this circumstance, so materially affecting his claims to a discovery which he evidently considers to be an important one.

But allow me, Sir, to examine the merits of this rule in its application to the *square* root. Let us suppose the boy was requested to extract the *square* root of the number 42436; here it is obvious the first figure of the root would be 2, and the last either 4 or 6;—if 4 be taken, then 4 or 9 would be found to be the *middle* figure; but if 6 be used, then 0 or 5 would be the *middle* figure; hence there would be no fewer than four different roots obtained by M. *Rallier's* method, of which four the boy could not possibly know the *correct* one, and he might assign either 206, 256, 244, or 294 for the root of the required number. This is no *particular* example, selected for the purpose of exhibiting M. *Rallier's* rule in the most *unfavourable point of view*; for it will be found upon trial, that had *any other number* been proposed, *four different results would have been obtained by this rule*; and that if a number ending with 5 had been proposed, no less than *ten different results would have been produced*, since all square numbers

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

bers ending with 5 will likewise terminate with 25, as I have shown in your Philosophical Journal, No. 99, where may also be seen some other curious properties relating to square numbers. It is manifest, therefore, that, if the boy adopted *this method,* he would not only make " many more errors in the extraction of the *square* than in that of the *cube* root ;" but that he would, in most cases, fail *three times out of four* ; and, in some cases, *nine times out of ten.*

Any of your readers may satisfy themselves respecting this ambiguity, by referring to a table of square numbers, where they will find that the *first* 25 *square numbers* contain all the varieties of the *two terminating figures* of such numbers ; and that the squares of all numbers *equally above and below* 25 ; as of 24 and 26 ; or of 23 and 27, &c. will have their two last figures the same : this property may not have been noticed by your readers in general, but those of them who are but slightly acquainted with mathematics may satisfy themselves of its truth and universality ; for since the difference of the squares of the sum, and difference of any two numbers is equal to four times the product of those numbers, it is manifest that the difference of the squares of two numbers of the form $25+a$, and $25-a$, would be of the form $100a$; that is, this difference would be *some exact multiple of* 100 ; and therefore two such squares could not differ in their *units* and *tens* places of figures ; viz. in their *two last digits* ; hence, then (since the *two* last figures *only* are used in M. *Rallier's* method) would arise the ambiguity which I have stated. It will be easily seen, that what I have shown of numbers of the form $25\pm a$, and $25-a$, is equally true of the general formulas $25n\pm a$ and $25n-a$.

Having proved, that M. *Rallier's* rule is only of *partial* utility in the extraction of the *cube* root, and of little or no use in the *square* root, " I think it would be *extremely un*fair to conclude, that either this method, or one very sinilar to it is adopted by the boy.

Suppose, however, Sir, that it were possible for the boy to have answered such questions as related merely to the *square and cube roots* of numbers by the help of the above rule, still this will not explain the method by which he *multiplies four figures by four, or by which he ascertains the factors of any number, however large, with a rapidity that has astonished some*

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of the first mathematicians in the country. I am aware, indeed, that Mr. A. refers to another memoir of M. *Rallier, on prime and composite numbers,* and I regret, in common with most of your readers, that he has not given us so much as a *single hint* respecting the method employed in this *second* memoir, though he says "it is probably the one pursued by the boy to find prime numbers, and to resolve numbers into their factors." Without knowing *myself*, however, what this method may be, I cannot think that it has been adopted by the boy, for several reasons; *first,* because it has been known for nearly *fifty years, secondly,* that none of the mathematicians who have seen the boy (except Mr. A.) have considered any of the *known* methods of operating with prime and composite numbers, as sufficient to account for the *rapidity with which the calculations have been performed*; and *thirdly,* that the method itself could never have fallen into *disrepute,* but would have been adopted not only by every *mathematician,* but by every teacher of *arithmetic in the most obscure country villages,* if it had been of such inestimable utility as to have enabled boys of ONLY SIX YEARS OF AGE *to have performed such astonishing calculations.*

Again, Sir, Mr. A. made no *new* discovery when he found that the boy, in extracting the square or cube root of any proposed number, made use only of the *two first* and *two last* figures. This curious and singular fact had been known for many months to several eminent mathematicians who had visited the boy, and who were soon convinced, from the *quickness* and *accuracy* of his answers, and from the power which he possessed of correcting himself whenever he committed an error, that M. *Rallier's* method was not the one he employed, *even in the extraction of roots,* much less in ascertaining the *factors of large numbers,* which he does with a *rapidity and apparent facility,* astonishing to those who have been long acquainted with the method alluded to, and who, notwithstanding their years of practice in abstruse calculations, find, that *they themselves* cannot perform such operations, neither by *that method,* nor by any other yet made public! What, then, shall we think of Mr. A.'s claims to the discovery of the "*modus operandi*?"

Mr. A. might have spared himself the trouble of [illegible] an *alteration in the intermediate figures of an* [illegible], which may be proposed to the boy, since such INTERMEDIATE FIGURES NEED NOT BE MENTIONED AT ALL; for it is well known, that, in a company of upwards of one hundred persons, amongst whom were some of the first literary and scientific characters in the kingdom, the following question was distinctly and unequivocally put to the child.—"Can you tell the *root* of a *perfect* cube number by means of the *two first* and *two last* digits *only*?" He answered "*Yes*:" and that the company might be satisfied that he *clearly understood the nature of the question,* it was put to him again in the following manner: "If a number of 12 *figures* be taken (which shall be a *perfect* cube) and the *two first* and the *two last* figures only be named to you, can you tell the cube root of the whole number?" To this he also replied, *Yes*. He was then tried by *various examples,* which he answered with a facility and correctness *that excited the wonder and admiration of every one present.* Now, Sir, was there in all this any appearance of *a wish to deceive?* any desire to *conceal any thing?* any fear expressed by the boy lest the various questions which were put to him might *lead to a detection of his method?* No, Sir, all was *fair, frank, open,* and *ingenuous*! But I am persuaded, Sir, that what I have stated must be sufficient to convince any unprejudiced person, that Mr. A. has not succeeded in *discovering the method* by which the child performs his operations; and I am therefore led to hope that I may thus counteract the tendency which the publication of Mr. A.'s letter in *your Journal* may probably have had to injure both the boy and his father. I am, Sir, with the warmest wishes for the success of your Journal,

Observations and facts in support of the talents, and originality of the methods of computing by Zerah Colburn.

Your most humble Servant,
W. SAINT.

Lower Close, Norwich,
March 13*th,* 1813.

VII.

METEOROLOGICAL JOURNAL.

1813.		Wind.	BAROMETER.			THERMOMETER.					
			Max.	Min.	Med.	Max.	Min.	Med.	Evap	Rain	
1st Mo.											
JAN.	24	N E	30 46	30·37	30·415	37	24	30 5			☾
	25	N E	30·47	30·45	30·460	36	29	32 5			
	26	N E	30 48	30 40	30·440	41	35	38·0			
	20	N	30 49	30 47	30 480	39	21	30·0			
	2	Var.	30 39	30·37	30 380	32	20	26·0			
	2	Var.	30·48	30 39	30·435	34	21	26 5			
	3	N W	30 48	30 44	30 460	42	30	36 0			
	31	N W	30·50	30·44	30 470	48	34	41·0		0 27	
2d Mo.											
FEB.	1	Var.	30 33	30·24	30·285	41	30	35·5			●
	2	N W	30 37	30·32	30·245	41	36	38·5			
	3	N W	30·45	30·37	30·410	43	34	38·5			
	4	W	30·45	30·29	30·370	41	34	37·5			
	5	S	30 29	29·78	30·035	47	36	41·5		—	
	6	S W	29·89	29·78	29·835	47	38	42·5			
	7	S W	29 98	29·79	29 885	48	37	42 5		—	
	8	S W	29·66	29·63	29·645	52	44	48 0		—	○
	9	S W	29·88	29 66	29·770	51	35	43·0		0·36	
	10	W	30·00	29·88	29·940	46	33	39·5			
	11	S	30·00	29 75	29·875	47	35	41·0			
	12	S	29·75	29·28	29·515	56	44	50·0		—	
	13	S W	29·48	29·37	29·425	57	39	48 0		0 33	
	14	S W	29 38	29 27	29·325	52	42	47·0		0·30	
	15	S W	29·34	29·27	29·305	52	41	46·5		0·18	☾
	16	S W	29 44	29·34	29·390	48	41	44·5		—	
	17	S W	29·37	29·30	29·335	52	43	47·5		0·27	
	18	S W	29·88	29·37	29·625	52	41	46·5		—	
	19	S	29·66	29 60	29·630	56	40	48·0		0·19	
	20	S W	29 80	29·66	29·730	53	42	47·5		—	
	21	S W	29·70	29·69	29·695	57	49	53·0			
			30·50	29·27	29·957	57	20	40·58		1·90	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

* REMARKS.

1813. *First Month.* 24. Light clouds and sunshine. 28. Rime on the trees: very misty a. m. clear p. m. 29. Hoar frost: the sky overcast. 30. Misty to the S. a.m. A grey day. 31. Misty a. m. Hoary *Cirrostratus* clouds: rain at night.

Second Month. 1. The *Cumulostratus,* which has not for a long time been exhibited, appeared to-day in large masses. 7. Showers and wind: at sun-set, several large clouds of the modification *Nimbus.* 8. Stormy. 9. A violent thunder gust from the west about 2 p. m. by which considerable damage was done to the roofs and chimnies of houses, &c. This was followed by a series of heavy gales (continuing with a few short intervals of calm and pleasant weather) to the end of the period. The *lunar halo* appeared before several of these, of a large diameter; and, on the 18th, about 11 a. m. there was a brilliant rainbow. The river Lea has considerably inundated the adjacent lands.

RESULTS.

Winds, in the fore part, northerly, with a very dry, dense air, and low temperature: in the latter part, southerly, with a rare and moist atmosphere, and high temperature.

Barometer: greatest observed elevation, 30·50 in.; least 29·27 in.

Mean of the period 29·957 inches.

* Thermometer: highest 57°; lowest 20°;

Mean of the period, 40·58.°

Rain 1·90 inches.

The current of evaporation has been again interrupted, and is therefore omitted, in order to be resumed in next report.

These observations will be continued at Tottenham, Middlesex, to which place the observer has removed his residence.

TOTTENHAM,
Second Month, 23, 1813.

VIII.

On the connection between Shooting-Stars and large Meteors, and proceeding both from terrestrial and satellitulæ, in rejoinder to Mr. G. J. Singer. By Mr. JOHN FAREY, *Sen.*

To William Nicholson, Esq.

SIR,

Phenomena of shooting-stars, and the means of observing them, &c.

BEING in the North of Scotland at the time that your September number appeared, and not having leisure and opportunity since to consult the same until now, I should not otherwise have delayed so long to reply to your correspondent Mr. G. J. Singer, were it only to repel an insinuation with which he concludes, viz. that I had in my 2nd letter to you on shooting stars, *exulted* in your valuable correspondent, Mr. Forster, having been deterred, or in his discontinuing to give his "accurate observations" of meteorological phenomena.

Accurate and sufficient observations on the phenomena of nature I am ever desirous of seeing multiplied as much as possible; not so I confess, those that are so loosely or incompletely made or recorded, as to lead only to the support of what I conceive to be a false hypothesis.

I have elsewhere and repeatedly recommended the multiplying of observations on shooting-stars and meteors, of a nature which does not seem to have occurred to Mr. S. viz. by two or more observers, at several miles distant from each other, each having a well-regulated watch, and a person stationed to read off, and record the observations made, in or near to some constellation previously fixed on by the observers, during a certain time each night, through a long period, the place, direction, and length of course, being recorded by each, with reference to a good planisphere of the part of the heavens fixed on, with which each observer is furnished; the comparative velocity, brightness, interrupting clouds, haziness, light of the moon, &c. &c. being recorded by each observer.

From a pretty complete series of observations thus conducted, in conjunction with my able friend Mr. Benjamin Bevan, now the engineer to the Grand Junction and other canals, and recorded during an hour at least, and oftener two or three on my part, resumed in *every evening* for more than a year; I am compelled to dissent from some of Mr. S.'s facts respecting these phenomena, and entirely so from his manner of accounting for them, by electricity, an agent which we well know was in fashion a few years ago, for explaining a great variety of phenomena.

Phenomena of shooting-stars, and the means of observing them, &c.

My observations, above alluded to, decidedly prove, that all which ought properly to be called *shooting-stars*, having only a short and rapid course, and but a small apparent size, appeared only when the air was *clear and cloudless*, in the place of observation, and when only a very small degree of *extraneous light* prevailed, and that the calculated place of many of these, from the best of our concurrent observations, made at 6 miles distant, shewing them to be at 60 to 100 miles distance from us, and at 40 to 50 miles of perpendicular height above the earth's surface, seemed exactly to accord with the short, feeble, and unobtrusive nature of their appearances.

My observations also shew, as I have mentioned, a very regular series, of what I denominate *meteors*, the least of which were nearly as short, quick, and faint in their appearances, as the shooting-stars, to others which were very long in their courses, slow in their motions (apparently) and with corresponding degrees of size and brightness, and attended by explosions and sparks, and small trains, some of the largest of them; but none of which last ever appeared but in clear parts of the sky: and our calculations shewed all of these last, to be far above the height of the ordinary clouds.

Mr. S. mistakes, apparently, in supposing me to have asserted, that the larger class of meteors are not sometimes visible on moon-light nights, and even before the day is quite closed, as I have more than once myself observed, but never *where* clouds appeared at the time, as we almost constantly see of flashes of *lightning*, the only visible *electric* luminous phenomena of the atmosphere, perhaps.

Whence Mr. S. gathered, that satellitic (not planetary or cometary) bodies, move "with *immeasurable* velocity," I am at a loss to think.

As Mr. Forster's authority has been referred to (though very inconclusively, I maintain) in page 34, I cannot help repeating, that if that gentleman, or Mr. L. Howard, ever did, or if they now concur with Mr. S. as to the shooting-stars being an

Phenomena of shooting-stars and the means of observing them, &c.

electrical phenomenon, they would in all probability have resumed the mention of them, and of other undisputed electric phenomena occurring at or near to the same time with them, in their subsequent and valuable papers or reports, in order to shew such their concurrence in Mr. S.'s opinion.

I have expressly mentioned, on different occasions, that it is the largest class of bursting *meteors*, or those having trains of stars after them, which are accompanied by falling meteoric stones, and not the shooting-stars; "*falling-stars*" I conceive to be an improper name for any of these phenomena.

The great comparative scarcity of meteors, and of the stars falling obliquely from them, indicative of their approaching dispersion and end, are sufficiently consistent, and prove nothing against my alleged connection of them with shooting-stars, an extremely frequent, and as I conceive, a periodical phenomenon. And here I would remark, that nearly all the meteors which I or my friends have seen, as well as the shooting stars, have vanished, or ceased to appear, almost at once, in *clear parts* of the sky, where they passed out of the atmosphere, as I conceive, and have not passed behind clouds, as has very commonly been said, in newspaper and other accounts. I have not "travelled" far enough, even in theory, to have said, or even conjectured, *where* the *satellitulæ*, more than the moon or large satellite of our planet, came from; but by no means can I admit the reasonings, that they came recently from the moon's volcanos, or more anciently from a burst planet, conjectured by some, to have given rise to Ceres, Pallas, and Juno.

I entirely dissent from Mr. S.'s 4th alleged fact, at page 36, and from the 4th, 5th, and 6th of those at page 37, as applied to shooting-stars. Mine is not a "*planetary* hypothesis," nor is it opposed, I believe, to either the facts or analogy of these, or other parts of the system of the universe, as now explained on the principles of the universal gravitation of masses, whose motions and present states are within the scope of our cognizance, but not their origins.

I remain, Sir,
Your obedient Servant,
JOHN FAREY, Sen.

12, Upper Crown Street, Westminster,
February 22d, 1813.

IX.

Account of a remarkable Appearance in the Ice of a Pond in which a man was drowned. (W. N.)

ON the west side of the road leading from Petworth to Chichester, at the distance of about four miles from that city, stands Halnaker House, formerly the seat of the Earl of Derby, by right of his wife, heiress of Sir William Morley, and more anciently of West, Lord de la War. On the west side of the park, are certain stables, and other buildings, inclosing a farm yard; in which is a pond of about 18 or 20 feet across, and 5 feet deep in the middle. A man was drowned in this pond in the month of November last; and the circumstances attending the discovery of his body were so curious and uncommon, that they occasioned much conversation at the time; but it does not appear that any probable explanation of their cause was pointed out.

Local situation of a pond in which a man was drowned in Halnaker Park, Sussex.

Very lately I was much gratified by a discussion of this subject in a select company of men of talents and observation, where we had the advantage of the facts being stated by the Rev. James Webber, Chaplain to the House of Commons, who was an eye witness. The clearness and precision with which this gentleman stated the events to us, gave a much more lively interest to the whole: for the narrative in the public prints, which had appeared most remarkable for its strangeness, and perhaps liable to doubt, now assumed the form of an authentic and accurate philosophical incident, capable of being examined and investigated. I did not scruple to request Mr. Webber to favour me with such written minutes as his own recollection, or enquiries among his friends, might afford, in order that I might communicate the same to my readers, with those deductions and remarks which, with the advantage of the conversation before mentioned, I might be enabled to make: and it is to his ready attention to my request, that I am obliged for the following statement, which I have made in my own words from his communication, and those of his brother, Mr. Archdeacon Webber, Vicar of Boxgrove, and the Rev. Mr. Valintine, Domestic Chaplain to the Lord Bishop of Chichester.

Authentic statement of the facts.

by respectable eye-witnesses.

About the 14th or 15th of December last, the pond in Halnaker Park, having been frozen over by the hard weather

The figure of a man was seen on the ice;

which

sufficient to disturb the crystallization of water into ice;—

—and generally to explain the present facts.

shew its effects by a perpendicular action from beneath the surface of the ground, and is capable of being modified by changes of no great extent in what may be lodged beneath. This power, though imperfectly understood, seemed sufficient to disturb a process so variable and delicate as that of crystallization; which we know is affected by the speedy or slow abstraction of heat or of moisture, the presence or absence of light, the action of tremors of any kind, and several other accidents;—and from a supposed difference in the agency of such a power upon the water immediately over the drowned man, and upon the other water, there appeared good grounds to account in a general way for the difference in their respective qualities.

More particular investigation.

My own reflections, since I had the pleasure of this conversation, will afford a few additional remarks, which may give a more specific form to this explanation. I am disposed to think that the effect was caused by the developement of heat in the body, occasioned by the putrefaction or chemical change which must have taken place, though it may have proceeded very slowly, on account of the coldness of the water, and the want of communication with the external air. This would occasion a small degree of expansion in the water, in contact with the body, and cause that fluid to ascend perpendicularly to the surface; where it would spread itself (thinly) on all sides, and when cooled would descend near the circumference, as is daily seen with fluids heated from beneath. Or, in other words, the particular mass of water, in this stagnant pond, which was perpendicularly over the corpse, and consequently had the same precise outline, as to all its horizontal sections, would be a little warmer than the rest. And though this circulation would be a little modified by the expansion to which water is subject, when cooled below 40°, yet this would scarcely in any case affect the result, and not at all, if the freezing came on suddenly. It appears from our meteorological tables that it did so, on the 11th December.

The body must have been slightly heated by its chemical change; and would warm the water above it; which would ascend, &c. having the outline of the figure.

The water exterior to the figure would freeze first confusedly, and form opake foul ice.

Now these simple facts might, and probably did, occasion the great differences in the ice. For the external water, being originally colder at the commencement of the frost, would sooner be cooled down to the freezing point, and begin to congeal over its whole surface, while the other part would remain fluid. The congelation being sudden, and from all points at once, would be of that kind which chemists call confused crystallization; forming an opake mass, and including the impurities of the liquid in its substance. But the water over the body being warmer, would not congeal at so early a period, except with regard to a few spiculæ, which would shoot into it from the surrounding ice, and form the white border of the figure; and when at last its surface became gradually cooled down to the freezing point, this water would freeze, not all at once, but by a slow crystallization, from the edge inwards; which is the very process for making clear ice, used by Achaed, in his electrical experiments, and is known to exclude all air bubbles, and mechanical mixtures of impurity, from the crystals.

The water over the body would freeze more slowly, and form a white border to the outline, and clear hard ice within.

These effects, indeed, suppose a concurrence of favourable circumstances which may rarely take place, but really appear to have met in this uncommon case, and seem to support the preceding explanation even in the minuter particulars. Thus the abrupt termination of the head of the figure, agreeing with the lower part of the hat, leads us to an inference, that the warmed water either found its way from under the hat, instead of rising from its upper border, or that the felt acted as a bad conductor of heat, to the surrounding water.: and the remarkably smooth slippery face of the ice of the figure, clear of snow, while the rest of the ice was rougher and covered with snow, should indicate that the rough ice was sufficiently cold to protect its snow from thawing by the day temperature, which in fact was *above* freezing; while the ice of the figure, though covered with snow on the 11th or 12th, was not so cold as to prevent that snow from melting in the day time, and again freezing in the night, with that very smooth and slippery surface which is noticed in the description.

The effects required a concurrence of circumstances, which appear to have taken place; and are confirmed by the minor incidents.

X.

Of the Caldeiras or hot fountains of the Furnas in the Island of St. Michael, one of the Azores.

PLATE VII. is taken, by permission, from "the History of the Azores," an entertaining work lately published, respecting a part of the globe but little attended to. The author's account of this phenomenon, states, that the columns of water are of very considerable diameter, and are thrown up to the height of about twelve feet. He did not take the degree of the heat, but from its effects it appears to have been equal to that of boiling. Sulphur rises in vapour through the surrounding ground, and the water itself is strongly sulphureous.

My correspondent R. B. has promised a Memoir upon the Theory of Hot Springs and Fountains, which I shall be glad to receive, and particularly that of the two alternately intermitting springs of Iceland, described by Sir G. S. Mackenzie.

SCIENTIFIC NEWS.

Geological Society.

March 5th, 1813.

The Right Hon. the Marquis of Landsdown,
The Right Hon. Charles Long, M. P.
William Clarke, Esq. Trinity College, Cambridge,
were severally elected members of the Society.

Two letters from Mr. Webster, Draughtsman, and Keeper of the Museum to the Society, addressed to L. Horner, Esq. were read.

In the first, Mr. W. states, that, during a late examination of the Isle of Wight, made by him for Sir H. Englefield, he discovered a series of calcareous strata of later formation than the chalk, especially characterized by containing fresh water shells. From this circumstance he was led to suspect a correspondence between this formation and the Calcaire d'eau douce, which has been described by Brugniart and Cuvier as forming some of the strata in the basin of Paris; which conjecture was confirmed by a comparison of the fresh water fossils of the Isle of Wight with those of the French strata, which were given by M. Brugniart to the Count de Bournon, and by him have been deposited in the cabinet of the Geological Society.

In consequence of this interesting discovery, the attendance of Mr. Webster at the Society's apartments was for a time dispensed with, that he might re-examine the Isle of Wight and its vicinity. Accordingly, his second letter is dated from Freshwater, in the Isle of Wight, March 3d. In this he states, that he has succeeded in obtaining some very desirable sections of the strata, and an abundant collection of specimens. He is inclined to think that there are two freshwater formations,

tions, with a marine formation between them. The lowest freshwater formation consists of beds of sand and marl, with numerous fragments of the Limnea of Lamarck, and of two species of Planorbis: the interposed marine deposit is of blue clay with Venuses, oysters, and various turbinated shells: and the upper freshwater formation consists of a calcareous rock, inclosing numerous and very fine specimens of the Limnea and Planorbis. Some of this last stratum is very friable, being only marl; other parts are extremely hard, and the uppermost part of it has a porcelaneous character. Mr. W. has not discovered any bed of gypsum, nor silicous concretions.

A paper by Dr. Mac Culloch on the Granite Tors of Devonshire and Cornwall, accompanied by three drawings, was read, and thanks were voted for the same.

The object of this communication is to show the process followed by nature in the destruction of the Granite rocks of Cornwall and Devonshire, and to explain how this process is dependent upon a particular mode of aggregation of the materials of which this rock consists, and which can be satisfactorily observed only during the progress of its decomposition.

Dr. M. first treats of the Granite which forms that promontary near the Land's-end on which the Loggan rock is situated. Its length is about 200 yards, and the entire rock of which it is composed is traversed by numerous vertical and horizontal fissures, thus dividing the mass into a multitude of cubical and prismatic blocks. The Loggan rock itself, which is the subject of one of the drawings, appears to be one of these blocks in a state of decay, but still occupying its original site. Its general figure is irregularly prismatic and four-sided, with a protuberance on the lower surface on which it is poised. The breadth of the apparent surface of contact between this protuberance and the rock that it rests upon, is about a foot and a half, but its figure being cylindrical, and not spheroidal, the motion of the stone is limited to a vibration in one direction. The utmost force of three persons (according to Mr. M.'s trials) is only capable of making its exterior edge describe an arc, the chord of which, at six feet distance from the centre of motion, is three quarters of an inch. A force of a very few pounds, however, is sufficient to begin and maintain a very visible degree of vibration. Even the wind, when blowing on its exposed western surface, produces this effect very sensibly. 'Its weight, as estimated from its dimensions, and the specific gravity of granite, appears to exceed 65 tons.

The subject of the second drawing is the *Cheese-wring*, which occupies the highest ridge of a hill to the north of Liskeard. It is an irregular column, about fifteen feet high, composed of five stones, the upper ones of which are so much larger than the rest as to overhang the base on all sides. The angles and external borders of these stones are considerably rounded by the effect of decomposition; and there is no doubt that in process of time, this disintegration will proceed so far, that the balance of the pile will be destroyed, and its ruins will not be distinguishable from the other bowldens, with which the tops of all the hills in this vicinity are overspread.

The third drawing represents the Vixen Tor on Dartmoor. Almost all the granite of Cornwall and Devon, like that of the Land's-end, is divided by fissures into masses more or less approaching a cubical form. If a rock of this kind, nearly level with the surface of the soil, is examined, the fissures will be found to be a mere mathematical plane, and the angles formed by their intersection will be sharp and perfect.

If we then turn our attention to granites, which, from their greater elevation above the soil, appear to have been longer exposed to air and weather, we may observe a gentle rounding of the angles, such as is exhibited in the Vixen Tor. By degrees the fissures become wider, and the blocks, which were originally prismatic, acquire an irregular curve-lined boundary, resembling those which form the Cheese-wring. If the centre of gravity chances to be high, and far removed from the perpendicular of its fulcrum, the stone falls from its support, and becomes rounder by the progress of decomposition, till it assumes one of the various spheroidal figures, which the granite bouldens so often exhibit.

These fissures, and the rounded form which the cubical blocks acquire by decomposition, Dr. M. is inclined to attribute to the original structure of the rock. In this, as in basalt, crystallization appears to have been begun in distinct, and more or less distant points, from each of which it proceeded, forming thick concentric lamellæ, till at length the exterior shells of adjacent concretions came in contact, but were incapable of mutual penetration. The outer lamellæ are the least hard and dense, and therefore yield the easiest to the various causes which occasion the disintegration of rocks.

March 19*th*, 1813.

Mr. Webster exhibited specimens of the rocks containing freshwater shells recently discovered by him in the Isle of Wight.

A paper on the rocks of Clovelly, in Devonshire, with illustrative drawings, by the Rev. I. J. Conybeare, was read, and thanks were voted for the same.

The fishing town of Clovelly is situated in a narrow ravine on the north coast of Devon, about 22 miles west of Ilfracombe. The shore is precipitous, being formed of cliffs about 130 or 140 feet in height, intersected by narrow alternations of Granwarke and Granwarke slate, curved and contorted in the most capricious way imaginable. No organic remains were observed in them, nor any foreign minerals, except opake white quartz, forming numerous veins. Nearly the whole of North Devonshire is composed of the rock just described, which is locally distinguished into *Dunstone and Shllat,* the latter being the slaty Granwarke, and the former the compact : it is always very irregular in its stratification, is destitute of metallic veins, alternates with transition limestone, and, where it does so, occasionally contains organic remains. It also, in one instance at least, alternates with thick beds of a kind of culm : its veins besides quartz occasionally offer calcareous spar. Killas, which Mr. C. is inclined to regard as a variety, not of nuca-slate, but of clay slate, is contorted in its stratification only in the neighbourhood of the Granwarke, is traversed almost through its whole extent by frequent veins or dykes of a porphyritic rock which does not pass into the Granwarke, contains sometimes topaz, and not unfrequently garnet : its veins are often filled with chlorite, mica, and crystallized filspar, and also contain tinstone, grey cobalt ore, &c. These characters, in the opinion of the author of this paper, form a sufficient mark of distinction between the Granwarke and the Killas of the West of England.

A paper on the Isle of Staffa, by Dr. Mac Culloch, was read, and thanks were voted for the same.

The circumference of this island is about two miles: it forms a kind of table land of irregular surface, gently sloping to the N. E. and is bounded on all sides by steep and generally perpendicular cliffs from 60 to 70 feet above high water mark, the greatest elevation in the island being about 120 feet.

The entire island is a mass of basalt, but a bed of sandstone is said to be visible at low water on the western side.

The basalt presents two varieties, the columnar and amorphous. the latter of which is generally amygdaloidal, containing zeolites. In the columnar variety lamellar stilbite is occasionally found filling the intervals of approximate columns, and sometimes, though rarely, in the substance of the smaller and more irregular columns.

On the south-western side of the island there appears to be three distinct beds of basalt, the lowermost of which seems to be amorphous. The next bed from 30 to 50 feet thick, consists of those large columns which form the most conspicuous feature of Staffa. The upper one appears at a distance to be a mass of amorphous basalt, but on closer inspection is found to consist of small columns, often curved, laid and entangled in every direction. The columnar basalt of Staffa is by no means so regular as that of the Giant's Causeway; but in return it presents many beautiful specimens of bending columns, which do not occur at the latter place; of these the most remarkable form a conical detached rock called Budchaille, or the Herdsman. Besides the great cave are two smaller ones, which being accessible only by a boat, and in perfectly calm weather can very rarely be examined.

The surface of the island is in some places overspread with a bed of alluvial matter, containing rounded fragments of granite gneiss, mica slate, quartz, and red sandstone, together with a few rolled pieces of basalt. As there is at present a considerable extent of deep sea between this bed and the nearest primitive rocks of Jona, Coll, Tirce and Mull, it becomes an interesting subject of speculation to inquire into the conditions requisite to account for this fact, there seems to be, either that a declivity sufficient to allow of the transportation of rounded fragments has formerly existed, sloping upwards from the present level of Staffa to some primitive mountains, which no longer exist, or that the whole island of Staffa itself has been raised to its present elevation from the bottom of the deep sea by which it is now surrounded.

Incombustible Cloth.

It appears, that the ancients had a method of making incombustible cloth of amianthus, which, notwithstanding the flexibility of its fibres has generally been considered as too brittle to be worked without a mixture of some other staple, such as flax, or cotton, to be afterwards burned out. Madame Perpenti has succeeded in working it with facility alone, after several trials from the writings of ancient authors.

Much depends, of course, upon the quality of the article itself. Her process consists, in softening the amianthus in water, beating it, rubbing it, and dividing with a double comb, with fine steel points. It is remarkable, that the fibres thus obtained are much longer than the solid piece, and may be had of the most extreme delicacy for fine fabrics. They are said to be as strong as those of silk or linen.

She has manufactured paper of this material, making use of gum to give consistence to the pulp. If an incombustible ink be required, the oxide of manganese would present itself as a preferable ingredient.

To Mr. Nicholson.

Sir,

Your correspondent O, in No. 158, will oblige a constant reader of your Journal to remove the following ambiguities in the extraction of the square and cube roots:

Sir,
Your obedient Servant,
B.

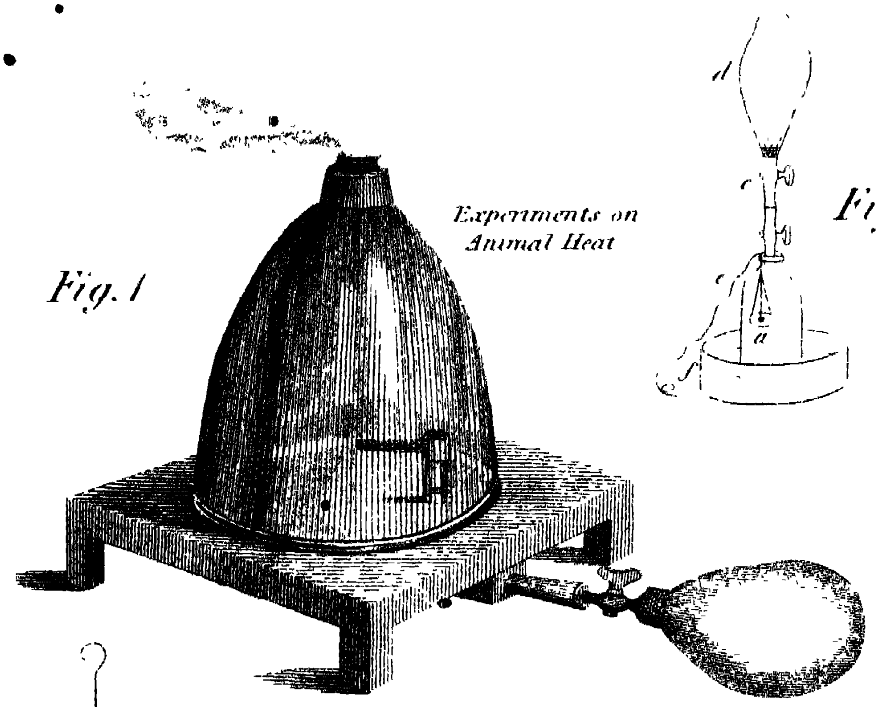
If the terminating figure of the power given be 1 or 6, t last figure of the *square* root may be 1 or 9, in the first, and 4 or 6 in the second case, how is the right figure to be ascertained ?

Again, 25, 672, 375—I want the *cube* root, according to the rule given $A=5$ and $B=5$—how is the process given by O to be applied here ? for from - - - 25
the millions given, if you deduct - - - 8
the cube of 2, the first figure of - - - —
the root, you have - - - 17
which is to be divided by the *two first figures* of thrice the square of 2—but $2\times2\times3=12$ (17)
12
—
5
1 is the quotient, but 9 is the number sought : how is this ?

Dr. Henry has requested the Editor to state, that Professor Bezelius, in his able sketch of the present state of animal chemistry, recently published, has committed an error in ascribing to Dr. H. the suggestion of the trial of magnesia in calculous diseases ; and that the merit of the hint which led to the successful experiments of Mr. Brande, belongs entirely to Mr. Hatchett.

Mr. Snart, Mathemetical Instrument Maker, 215, Tooley Street, Southwark, has shewn me by a letter, that he invented the Chondrometer described in our last, and supplies the scale-makers and others with them, who have no pretensions to the contrivance. He has likewise had the candour to remark, that he subsequently found, that the same had been used several years before.

Fig. 1



Experiments on Animal Heat

Fig. 3

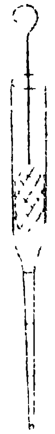
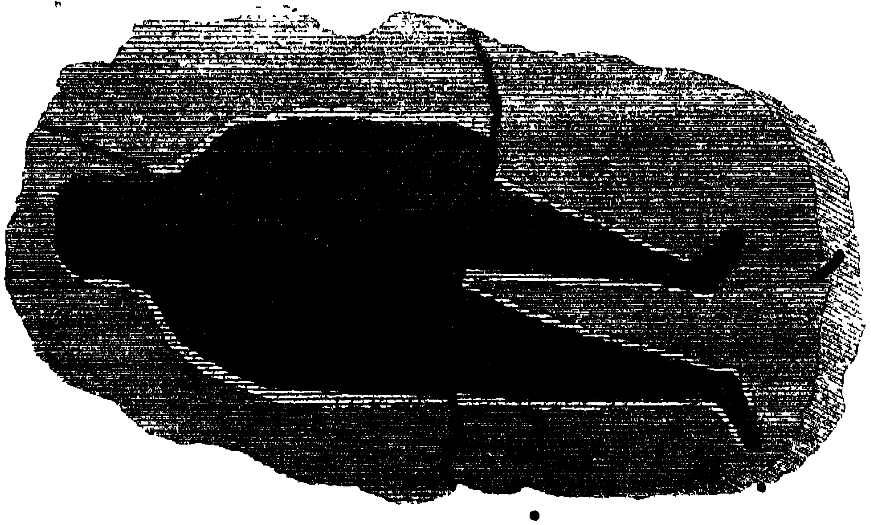
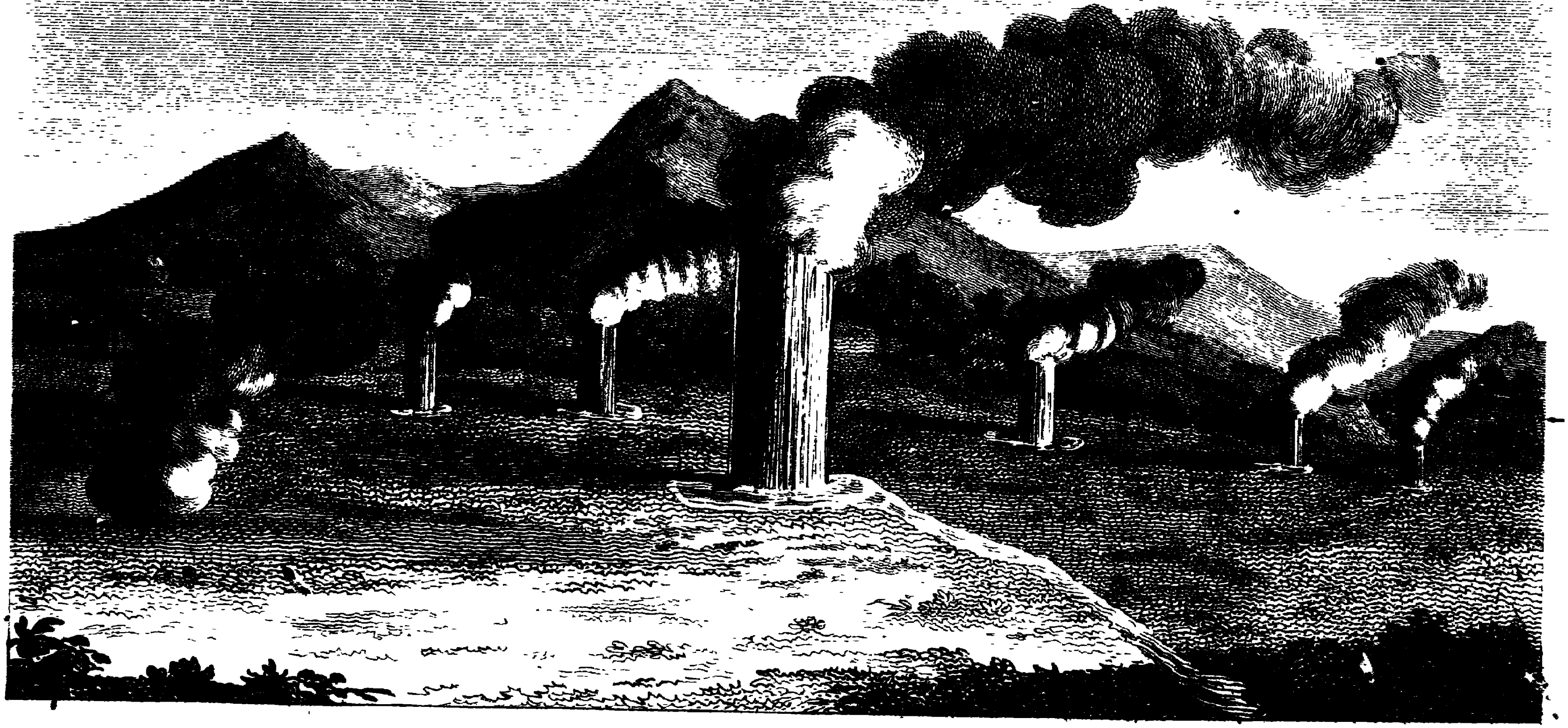


Fig. 4

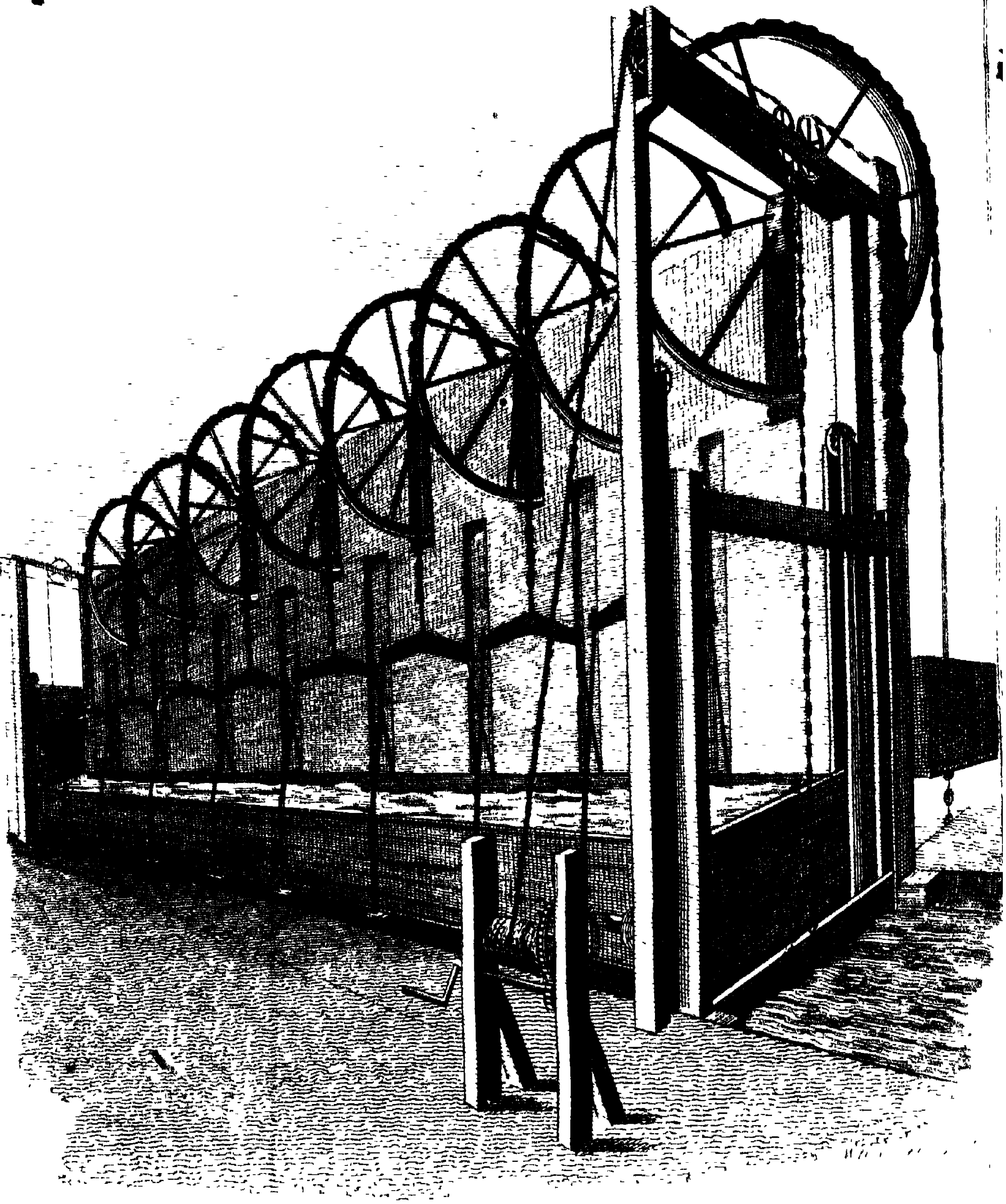
Singular Phenomenon in the Ice beneath which a Man lay drowned. Fig. 2



Journal Pl VII Vol XXXIV p. 312.



Loder Sc.



THE PERPENDICULAR LIFT

of Mr. Woodhouse.

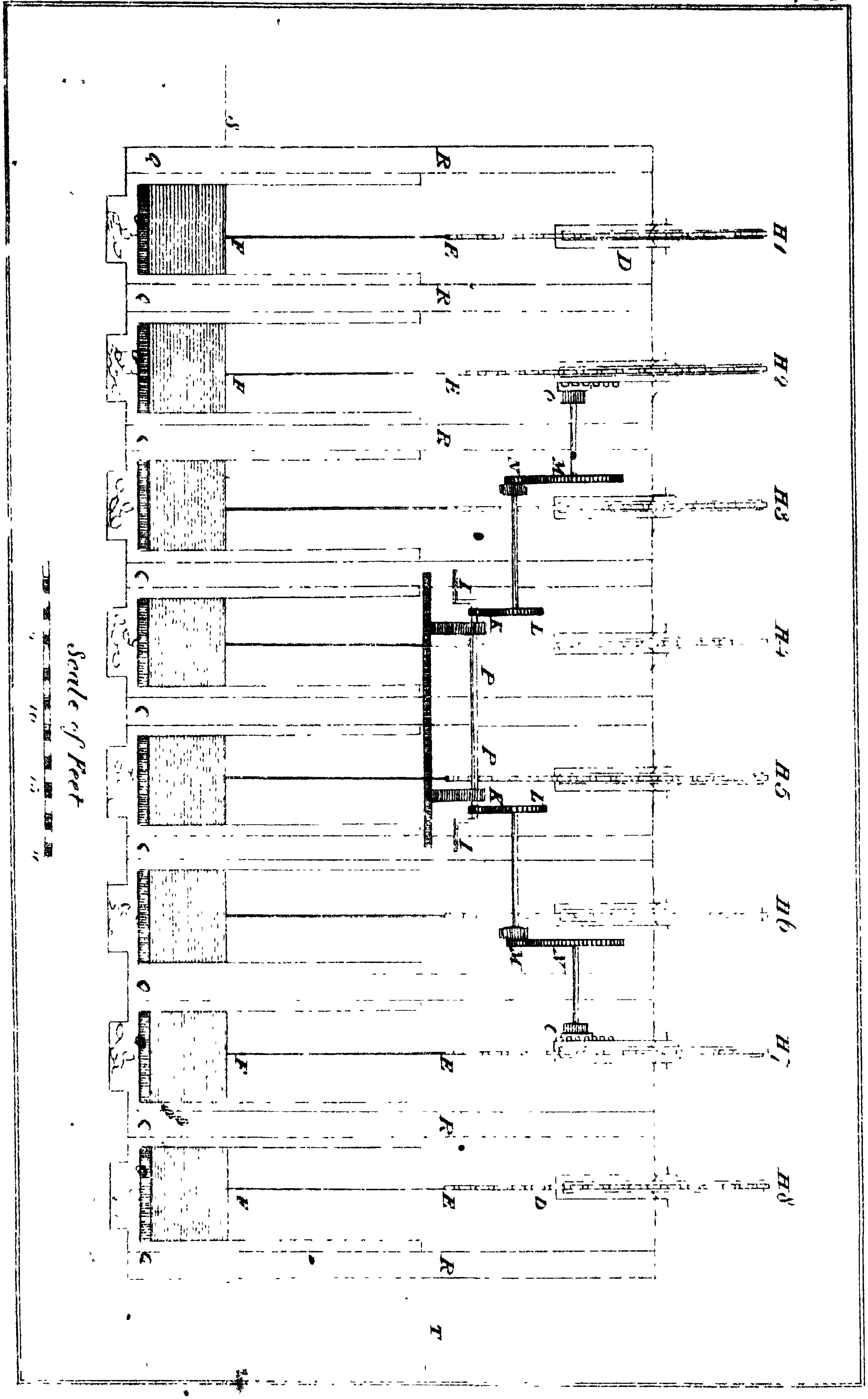

H1
H2
H3
H4
H5
H6
H7
H8
S
R
E
F
D
C
M
N
L
K
I
P
T
Scale of Feet
10
20

FIG. 1.

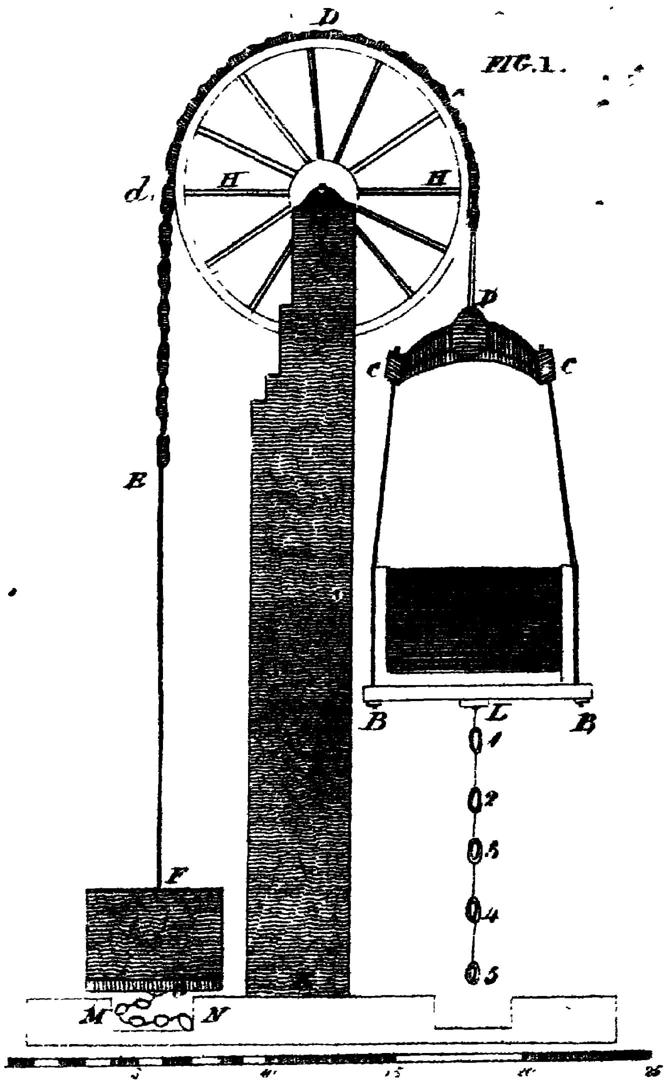


FIG. 2.

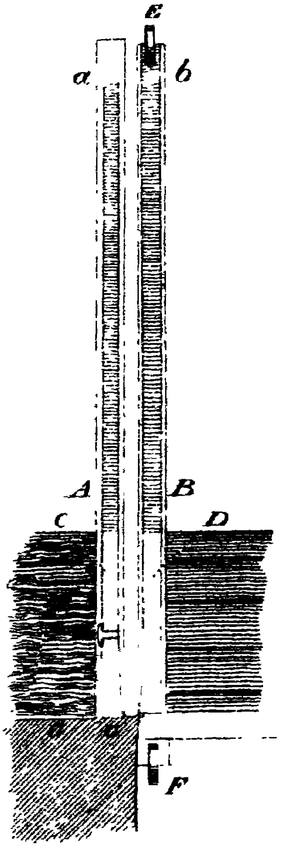


FIG. 3.

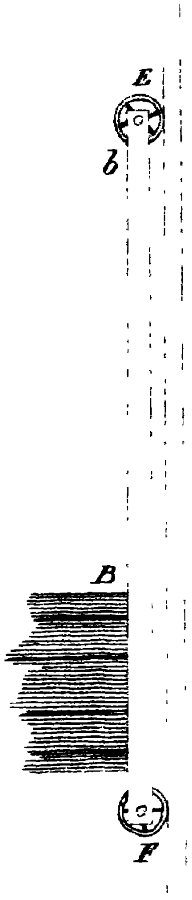
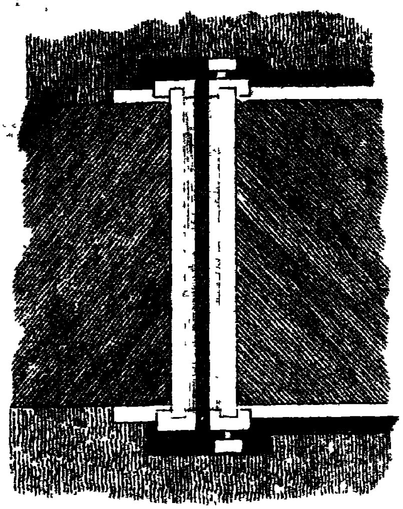


FIG. 4.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXXIV.

ARTICLE I.

An explanatory Statement of the Notions or Principles upon which the systematic Arrangement is founded, which was adopted as the Basis of an Essay on Chemical Nomenclature. By Professor J. BERZELIUS.

(*Continued from p.* 246.)

The proportions of oxigen and of sulphur in antim. compounds are the same as with other metals.

IF we admit that 100 p. of antimony form the white oxide with 27·8 p. of oxigen, and the sulphuret with rather more than 37 p. of sulphur, this result corresponds very well with these data, and proves that the white oxide contains 1½ times as much oxigen as the fusible oxide, and that the sulphur in the sulphuret is to the oxigen in the fusible oxide in the same proportion as we have found with the other metals. For if, in this case, the white oxide be composed of 78·25 p. of metal, and 21·75 of oxigen, this last, in the experiment I have mentioned, was replaced by 29 p. of sulphur—that is to say, 7·25 p. more than the weight of the oxigen; and these 7·25 p. making precisely one-third more than the weight of the oxigen, and the antimony is to the sulphur, in this experiment, as 100 : 37·07.

If any one among the experiments had produced a very decided result, we might easily have calculated the others from it;

but at present we must be content with the mean or average numbers.

5. Yellow oxide of antimony. Antim. digested with nitro-muriat. acid or fuming nitrous acid, the dried mass is yellow.

5. *Yellow oxide of antimony.* If antimony be digested with the nitro-muriatic acid, or with fuming nitrous acid, and the solution be evaporated to dryness, and then sufficiently heated to drive off the acids without igniting the mass, a yellow powder is obtained, which is often crystalline, and, if strongly heated in the fire, becomes white, and consequently did not owe its (yellow) colour to an adulteration with the red oxide of iron.

100 p. of metal gave 130 oxide.

When I made this experiment in a glass phial, weighed, and made use of the fuming nitrous acid, I obtained as much as 131 p. of a yellow crystalline powder; but when I had oxided the metal by means of the nitro-muriatic acid, the result did not exceed 129 or 130 p. of oxide from 100 p. of metal, and new cohobations of acid upon the yellow oxide did not increase the weight. The addition of 30 or 31 p. of oxigen does not agree with any proportion of those which I had before found, and I began to suspect a combination of two different degrees of oxidation had been formed, and that this combination could not be farther oxided by means of acids.

supposed to be a compound of two different degrees of oxidation,

I shall not mention the various unsuccessful experiments I made to clear up this intricate subject, but shall speak only of those which afforded more satisfactory results.

Antim. oxided by red oxide of mercury,

I mixed powdered antimony with the red oxide of mercury, prepared from very pure mercury, and heated the mixture in a glass retort. At a certain temperature it took fire, and the mass became red. I continued the heat as long as any mercury was condensed in the neck of the retort; after which, at length, there remained a deep olive-coloured powder. I heated this with great care in a glass capsule, by which treatment it lost its olive colour, and left a straw-coloured powder in the capsule.

gave straw-coloured powder.

The yellow oxide loses one fourth of its oxigen by heat, and becomes white oxide.

One hundred parts of this powder very strongly heated in a platina crucible, left, in different experiments, from 33·5 to 33·75 parts of white oxide of antimony. In order to determine the nature of what had been dissipated by the heat, the experiment with the yellow oxide was repeated in a small glass retort, with a small apparatus for collecting the gas. When the retort began to be strongly ignited, a gas was developed, and when this ceased the retort was removed from the fire. The residue was white oxide, and the gas very pure oxigen. These experiments prove, that the yellow oxide is decomposed by the heat by losing part of its oxigen; and as 93·75 p. of white oxide contain 20·24 of oxigen, it is clear, that the yellow oxide loses, on this occasion, one-fourth of its oxigen, and that it contains $1\frac{3}{4}$ as much oxigen as the white oxide, and twice as much as the fusible oxide (oxidum stibiosum.)

Other methods of producing yellow oxide (*a*) by mere heat;

There are several other methods of producing the yellow oxide (*a*) If into a long-necked tubulated phial a certain quantity of metallic antimony be put, and heat be applied to a cherry redness, and continued four or five hours, the antimony gradually combines with oxigen; a small quantity of white oxide sublimes into the neck of the glass, another portion escapes out of the aperture; but the greatest part of the oxided metal forms a yellow crust round the border of the fused metal. In this form the yellow oxide is not crystallized; it has much tenacity, and is difficult to break.

(*b*) by combustion with nitre.

(*b*) If one part of antimony in powder be burned with 6 p. of nitre in a red hot crucible, the burned mass is then decomposed with nitric acid, which leaves a white powder not dissolved. This powder is a combination of the yellow oxide with water, which may be driven off by heat over a spirit lamp. The oxide is then left of a very clear and fine yellow colour.

Degrees of oxidation stated in series 1,—,4, 6, 8.

The degrees of oxidation of antimony may therefore be expressed by the series 1, $1\frac{1}{2}$, 2 : but we have not comprehended the suboxide in this series, which contains less oxigen than the first of the series. If, from what I have endeavoured to make probable with regard to sulphur and arsenic, the quantity of oxigen in the suboxide be one-sixth of that in the white oxide, or one-fourth of the fusible oxide, the series will be 1,—, 4, 6, 8. If, on the contrary, future experiments should prove, that the oxigen in the suboxide is only, in fact, half that of the fusible oxide, the series will become 1, 2, 3, 4.

Although it is very clear, that the middle numbers must be deficient in precision, particularly when, at the same time, they are not founded upon good experiment, yet we must, amidst results so little fixed, content ourselves with an approximation of this nature. And by assuming, that the oxidum stibiosum is composed of 100 parts of metal, and 18·6 p. of oxigen; and that the suboxide contains with 100 parts of metal one-fourth part as much, the oxides of antimony will be composed as under.

Table of the proportions of oxigen in the four oxides of antimony.

	Met.	Oxigen.	Metal.	Oxigen.	Metal.	Ox.
The suboxide	100	4·65	96·326	3·174	2150·65	100
The fusible ox.	100	18·60	84 317	15·683	537·63	100
The white ox.	100	27·90	78· 19	21·810	358·41	100
The yellow ox.	100	37·20	72·85	27·150	268 81	100

Yellow oxide by n. acid appears to be composed of the white and yellow oxides, in portions containing equal quantities of oxigen.

Now, if we calculate the composition of the yellow oxide obtained by the action of nitric acid upon antimony, we shall find it such as it would be if composed of the white oxide and the yellow oxide, in such proportions as that each of them should contain an equal quantity of oxigen*.

[*The following annotation, on account of its length, is continued in the body of the Memoir.*

Other instances of compounds of two oxides, each of which appears to contain the same quantity of oxigen; and serve as the bases of compound salts.

* In proportion as our researches shall be extended, we shall find abundance of facts of this nature. In my first essay upon determinate proportions, I had the notion, that the difference of colour in precipitates, caused by the alkalis in the solutions of iron depended on a combination of two degrees of oxidation of iron, which probably afforded salts with excess of base, and I gave examples in the nomenclature by the prussias ferroso-ferricus, sulfas ferroso-ferricus, &c. One circumstance, which strikingly proves these notions, is found in the phosphas ferrosus. By precipitating sulphas ferrosus by means of a neutral phosphate, a white precipitate is obtained; and the same happens by precipitating the phosphas ferricus. But the first, the phosphas ferrosus, acquires a fine blue colour by the washing, which does not happen with the latter. In fact, a blue precipitate is obtained by the alkaline phosphates, in a mixture of the sulphas ferrosus and sulphas ferricus. The blue phosphate of iron is, therefore, a salt with double basis, containing the two oxides of iron, representing two different bases, and it constitutes a phosphas ferroso ferricus. The phosphas ferrosus, on the contrary, is white, as well as the phosphas ferricus. I have reasons to think, that several metals, viz. manganese, uranium, cerium, and mercury, have the property of producing salts with double bases, by combining their two degrees of oxidation. But it is, no doubt, still more interest-

Metals possess this habitude.

ing to find that the same kind of combinations likewise exists between the different degrees of oxidification of the same radical. It is known, that M. Gay Lussac, by extremely interesting researches, has found that 100 p. in volume of azote gas combine with 50 p. in volume of oxigen gas, to form the nitrous oxide,—that with 100 p. of oxigen they form nitrous gas, and with 200 p. nitric acid. The series would here be 1, 2, $3\frac{1}{3}$, 4, which is contrary to all analogy with other bodies. The irregularity of this series did not escape the attention of this learned chemist; but he endeavoured to remedy it by considering nitrous acid as composed of three volumes of nitrous gas, and one volume of oxigen gas. The analogy with the other oxides combined with the results of my experiments on the nitrites is here, in fact, 1, 2, 3, 4; and that the nitrous oxide, such as it is found in the nitrites, is composed of 100 p. in volume of azote, and 150 p. in volume of oxigen. But if we consider that, probably, neither the nitrous nor the nitric acid are capable of existing together in insulated states, as is also the case with many other acids; and that when nitrous acid is produced, the result would be a combination of the nitrous with the nitric acid as oxided bodies, and in such proportion that each shall contain an equal quantity of oxigen, the quantity of oxigen absorbed will be precisely what was found by M. Gay Lussac.

The same doctrine applied to the bases of acids; e. g. azote and oxigen.

(Here the annotation ends)

A very interesting question remains to be considered respecting the oxides of antimony. What is their chemical nature? Is the yellow oxide a superoxide, or has it the properties of an acid? The following experiments will prove, that the yellow oxide, as well as the blue, possess the characters of acids, and that the yellow oxide may be considered as an acidum stibicum, and the blue as an acidum stibiosum.

The yellow and the blue oxides of antimony have the characters of acids.

(a) *Combinations of the acidum stibicum with saline bases, stibiates.*

I burned 10 grammes of antimony in powder, with 60 grammes of pure nitre in a silver crucible, and the mass was heated during an hour in the highest temperature the vessel could support. I pulverized the white mass which was thus obtained, washed it with cold water as long as any nitrate of potash

Combinations of ac. stibicum (or yellow oxide) with saline bases. *Stibiates.* Stibiate of potash.

potash could be so dissolved; a white powder was then left, which I dried upon blotting paper. One part of this powder boiled for some hours with water was dissolved, and I passed the solution through the filter. The filtered liquid had a slightly bitter taste, rather metallic. It restored the blue of turnsole paper, which had been reddened by a weak acid, and the smallest quantity of a diluted acid caused a precipitate which was not redissolved in the fluid. The precipitate afforded by the acetic acid, well washed, was white and tasteless; but it reddened turnsole paper, although the filtered solution had not yet lost the property of acting like a weak solution of alkali. The white precipitate caused by passing a current of carbonic acid gas through the alkaline liquor, had the same property of reddening turnsole paper; a property which consequently belongs to the precipitated oxide, and cannot be owing to the combination with the acid, which served to precipitate it. The dry precipitate did not, in several months, lose the property of re-acting like an acid upon vegetable colours. But when I heated it in a retort over the flame of a spirit lamp, a quantity of water was disengaged, which did not redden them, and was very pure; and the yellow oxide which remained had lost the property of reddening turnsole paper.

deprived of its potash leaves an hydric stibiate.

One part of the saline solution, evaporated in a silver crucible, left a white mass. I decomposed this by digesting it with diluted nitric acid, by successive operations, to extract the potash. The undissolved oxide was white. I washed it with much water, and at last dried it at the temperature of boiling water; and then I heated certain portions, previously weighed, in order first to drive off the remaining water, and next the oxigen, until the white oxide only should remain. In several experiments 100 parts left 88·3, 88·7 and 88·9 p. of white oxide. The quantity of oxigen combined with 88·7 p. of the white oxide, in order to form the yellow oxide, is, from what we have determined, 6·45; and therefore the quantity of water was only 4°78. But 95·22 p. of yellow oxide contain 25·81 p. of oxigen, and 4·78 of water contain 4·218; now, 4·218 × 6=25·3 It follows, therefore, that the white powder not dissolved by the nitric acid, is an *hydrique stibiate*, that is to say, a combination of stibic oxide, with water, as its base, and in which the acid contains six times as much oxigen as water.

The nitric acid by which the stibic acid was separated from the saline mass, contains nitrate of potash. This saline mass was, therefore, in fact, a stibiate of potash.

(To be continued.)

II.

Inquiries relative to the Structure of Wood, the specific Gravity of its solid Parts, and the Quantity of Liquids and elastic Fluids contained in it under various Circumstances; the Quantity of Charcoal to be obtained from it; and the Quantity of Heat produced by its Combustion. By COUNT RUMFORD *F. R. S. Foreign Associate of the Imperial Institute of France, &c.*[*]

SINCE the days of Grew and Malpighi, there have been but few regular inquiries into the structure of wood. The science of botany has, indeed, taken an excursive range; and the indefatigable zeal of modern naturalists, who have travelled over all the known world, has made us acquainted with an astonishing number of plants, unknown before in Europe, and therefore called *new*, by which our gardens and apartments are embellished with a profusion of gay flowers; but still the knowledge of the vegetable economy is scarcely at all advanced. The circulation of the sap in plants is still a subject of dispute, and the causes of its ascension are very imperfectly known. The specific gravity of the solid parts which form the wood of plants, is unascertained, and, by consequence, the proportions of solids, of liquids, and of elastic fluids; the component parts of a plant, with the variations to which they are subject in different seasons, are matters of which we are still ignorant.

It is, indeed, known, that the wood of a tree remains and preserves its primitive form after it has been converted into charcoal; but no one has explained this extraordinary phenomenon, very little attention having been paid to it.

An earthen vessel becomes hard and brittle in the potter's furnace; the vessel shrinks during the operation of baking, but it undergoes no alteration of shape. This phenomenon is easily accounted for : the water which distended the particles of the clay, kept them at a distance from each other, and rendered the mass soft and flexible, having been expelled by the power of the heat, the several particles contract themselves together, and form a hard and brittle body, though the clay remains the same before and after the operation.

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[*] Read at the sittings of the first class of the Institute, September 28, and October 5, 1812.

Is it not possible that wood is converted into charcoal by a similar process? For, either the charcoal is already formed in the wood, or the wood being decomposed, the charcoal is formed of its elements, or a part of them. But is it not evidently impossible that the elements of a solid body should be so totally deranged as to separate them entirely from each other without destroying the form or figure of the body ?

In the sequel of this paper it will be shewn, that the specific gravity of the solid parts of any kind of wood is very nearly the same as that of the charcoal obtained from it, a circumstance that gives a great degree of probability to the hypothesis, that the two substances are identically the same.

But I do not mean to amuse the Class with a detail of my own conjectures ; it is to my experiments and their results that I now claim the honour of calling its attention.

I was by accident first induced to enter upon this examination and inquiry into the structure of wood. In the course of a long series of researches upon heat, I wished to determine the quantities of that element produced by the combustion of different kinds of wood ; but I had scarcely began the inquiry when I found, that in order to procure satisfactory results to my experiments, it was indispensably necessary to obtain a better knowledge of wood itself; and, therefore, I immediately devoted myself to the study.

My first aim was to determine the specific gravity of the solid parts which compose the fabric of the wood, in order afterwards to determine the quantities of sap or water contained in wood under various circumstances.

Having found, that very thin shavings filled with sap, or even with water, could be thoroughly dried in less than an hour, without injury to the wood, in a stove kept at a higher temperature than that of boiling water, or at about 50°

of Fahrenheit's scale, (= 260° French) I determined on using shavings of this description in my experiments.

SECTION I.

Of the specific Gravity of the solid Parts of Wood.

Specific gravity of the parts of wood, &c.

I began with the wood of the lime-tree, of which the texture is very fine and regular. From a small board, five inches long and half an inch thick, very dry, I took a quantity of thin shavings with a very sharp plane. These were exposed for eight days in the month of January, upon a table in a large room, not otherwise occupied, in order that they might attract from the atmosphere all that moisture which, as an hygrometric body, they were capable of imbibing. The temperature of the room was about 46 F.

Ten grammes (154·5 grs.) of those shavings, laid on a China plate, were placed in a large stove made of sheet-iron, and there exposed to a regular heat of about 245° F. for two hours, in the course of which time they were frequently taken out and weighed, in order to observe the progress of their dissication. When they ceased to lose weight, the operation was stopped; when perfectly dried, their weight was 8·121 grammes.

By previous trials with my apparatus, I had learned, that if the stove was too much heated, the shavings became discoloured, which is always indicated by the emission of a particular odour, very readily to be perceived; but, by a careful regulation of the fire, this accident may be avoided, and the shavings be thoroughly dried without injury, or even subjecting them to any sensible alteration.

I concluded that they had not undergone any change, because, upon again exposing them to the atmosphere, they regained the same weight which they had, under similar circumstances, prior to their being dried in the stove.

Being thus possessed of the weight of my shavings, as well under exposure to the air as in a dried state, which latter I could not but look upon as being perfect, it only remained to ascertain their weight in water when all their vessels and pores were completely filled with that liquid, to enable me to deter-

Specific gravities of wood, &c.

mine the specific gravity of the solid parts of this wood, which was accomplished without difficulty by the following process:

A cylinder copper vessel, ten inches in diameter, and as many deep, was filled with water from the Seine, previously well filtered, and being set upon a common chafing dish, was made to boil for some time, to expel the air contained in the water. The shavings were then thrown into the boiling water, and kept in that state for an hour. The water was not long in filling the vessels and pores of the shavings, from which it dislodged the air contained in them; so that the wood, specifically heavier than the water, was precipitated to the bottom of the vessel, and there remained.

When the vessel was removed from the chafing-dish, the water was suffered to cool to the temperature of 60° F. and then plunging in both hands, I placed (under the water) all the shavings in a cylindric glass vase, whose weight I had previously ascertained, which was suspended in the water by a silken cord, fastened at its other extremity to the arm of an accurate hydrostatic balance.

On weighing the shavings in the glass vase thus immersed, I found their weight equal to 2·651 grammes.

As the shavings, while dry, weighed 8·121 grammes, in the air, and 2·651 grammes in the water, they must have lost 5·47 grammes of their weight in the latter; consequently, they must have displaced 5·47 grammes of water; and the specific gravity of the solid parts of this wood must be to that of the water, at the temperature of 60° F. as 8·121 to 5·47, or as 14·846 to 10·000.

It may, perhaps, excite some surprise, that the solid parts of so light a wood as that of the lime-tree should be heavier, by nearly one-half, than water, taken in equal bulks. But this surprise will, without doubt, be increased when I declare, that the specific gravities of the solid parts of all kinds of wood are so nearly alike, as almost to induce a belief, that there is the same identity in the ligneous substance of all sorts of wood as in the osseous substance of all species of animals.

I procured, from a joiner's work-shop, dried wood of the eight following species, viz. poplar, lime, birch, fir, maple, beech, elm, and oak; and had them cut into small boards five inches in length, and six inches broad, from each of which

Specific gravities of wood, &c.

I planed off some thin shavings, and exposed them to the air for eight days, in the month of January, in a large room, where the temperature, which varied but little, was about 40° to 45° F.

When these shavings had acquired their ordinary degree of dryness under existing circumstances, ten grammes of each sort were weighed off, and, being laid separately in China plates, were thoroughly dried in the stove.

On being taken out of the stove, they were again weighed, and then thrown into boiling water, to expel the air from their pores, and to moisten them thoroughly. When they had boiled for an hour, they were suffered to remain in the liquor till it was sufficiently cool; and after they had been weighed in the water, the specific gravity of their solids was calculated in the usual way.

The following table gives the details and results of this inquiry.

Species of wood.	Weight.			Specific gravity of the solid parts of the wood.	Weight of a cubic inch of the solid parts of the wood.
	Exposed to the air in a room in winter.	Thoroughly dried in a stove.	In the water at 60° F.		
	grammes		grammes		grammes
Poplar	10	8·045	2·629	14854	29·45
Lime	10	8·121	2·651	14846	29·40
Birch	10	8·062	2·632	14848	29·44
Fir	10	8·247	2·601	14621	28·96
Maple	10	8·137	2·563	14599	28·93
Beech	10	8·144	2·832	15284	30·30
Elm	10	8·180	2·793	15186	30·11
Oak	10	8·336	2·905	15344	30·42
		Water		10000	19·83

The specific weight of the solid matter which composes the fabric of these woods is so nearly alike in them all, that the small variations to be observed in the different experiments may, perhaps, be accounted for otherwise than by supposing the ligneous substance to be essentially different in the several species.

The charcoal obtained from the various kinds of wood, if carefully prepared, has no sensible difference; and all the seer-woods give nearly the same chemical results when treated in the same manner. Hence, without doubt, we have good reason to suspect, that the ligneous substance of all woods is identical. But without stopping to discuss this question at present, I shall endeavour to elucidate another, no less interesting, and which yields results more satisfactory.

SECTION II.

Of the Quantities of Sap and Air discovered in Trees, and in Seer-woods.

Sap, &c. in different woods.

Greeve and Malpighi discovered in plants certain vessels, which they suspected to be destined for the reception of air; and many physiologists have supposed, that the air found shut up in the vessels of plants, which (if it be really confined there) would necessarily cause a reaction upon the neighbouring vessels, with an elastic force, as variable as the temperature to which this elastic fluid is exposed, and might probably contribute to the circulation of the sap.

It would, doubtless, be an interesting question to determine precisely the quantity of air contained in plants in different seasons, and under various circumstances. By examining the variations to which this quantity of air is subjected, and combining them with other simultaneous phenomena, we might hope to make some discovery which may assist us a little to elucidate the profound obscurity that at present conceals this part of the vegetable economy.

The specific gravity of the solid parts of a plant being known, it becomes very easy to determine, in every case, the quantity of air contained in its vessels and pores.

The following example will render this position perfectly clear:

An oak, in complete health, in a growing state, was cut down on the 6th of September, 1812. A cylindrical piece, six inches long, and rather more than an inch in diameter, taken from the middle of the trunk of this young tree, three feet above the earth, weighed, when full of sap, 181·57 grammes.

Upon plunging this piece of wood into a cylindric vessel about 1½ inch in diameter, and 6¼ inches in height, filled with water at the temperature of 62° F. it displaced 188·57 grammes of the

Sap, &c. in different kinds of wood.

the water*; whence I conclude with certainty that this piece of oak, filled with sap, possessed a bulk equal to 9·5093 cubic inches, that its specific gravity was 96515, and, consequently, that a cubic inch of it weighed 19·134 grammes.

When the piece of wood had been reduced to the shape of a small board, about half an inch in thickness, I took from it forty very thin shavings weighing 19·9 grammes, but when thoroughly dried in the stove, at a temperature of 262° F. they weighed only 12·45 grammes.

From this experiment, it is evident that the wood in question, being full of sap, was composed of 12·45 ligneous parts, and 7·45 parts of water, or of sap, whose specific gravity is nearly the same as that of water.

Now, as one cubic inch of this wood weighed 19·134 grammes, it is very certain that it was composed of 11·971 grammes of ligneous parts, which were, consequently, solids, and of 7·163 grammes of sap.

But we have already seen, from the results of the experiments detailed in the former part of this memoir†, that a cubic inch of the solid parts of the wood of the oak, weighs 30·42 grammes; consequently, the 11·971 grammes of solid parts

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* In order to determine and keep an account of the quantity of water remaining on the surface of this piece of wood at the instant of withdrawing it from the vessel, it was weighed when taken out, whilst still quite wet. As its weight had been taken previously to the operation, the augmentation it had acquired from the water was ascertained to a nicety.

The vessel when empty weighed 188·22 grammes, and when filled with water at the temperature of 60 F. 474·9 grammes; so that it contained 286·68 grammes of water. When the piece of wood was plunged into the water, a small glass plate, about two inches in diameter, and two lines in thickness, ground with emery, to fit it to the edges of the vessel, so as to close it hermetically, was laid upon its mouth to shut up the piece of wood with the water still remaining in the vessel, whilst its outside was wiped with a dry cloth.

When the exterior of the vessel had been thoroughly dried, the glass cover was carefully removed, and the piece of wood withdrawn; the vessel was then weighed again with its remaining contents of water: and from its weight the quantity of water displaced by the wood, was calculated.

† See the table, page 323.

Sap, &c. in different kinds of wood.

found in one cubic inch of this wood, when the tree was alive; could have no greater bulk than..........	0·39353 cubic inch.
As one cubic inch of water weighs 19·83 grammes, the 7·163 grammes of sap, found in the cubic inch of this wood, must have occupied a bulk equal to....	0·36122
Consequently, a cubic inch of the wood in question, contained a quantity of air, whose bulk was equal to.............	0·24525
Making together............	1·00000 cubic inch.

We conclude from these results, that a young oak, in a growing state, at the beginning of September, when the wood appears to be diffused with sap, contains, nevertheless, about a fourth of its bulk of air, and that its solid ligneous parts do not make quite 4-10ths of its bulk. But we shall presently see that the lighter woods contain still less of ligneous parts, and more of air, than the oak.

A young Italian poplar, three inches in diameter, measured at two feet above the earth, was cut down on the 6th of September, whilst the tree appeared to be in a growing state. The specific gravity of a piece taken from the middle of the trunk, was found to be 57·946; consequently, a cubic inch of this wood weighed 11·49 grammes.

From a piece of this wood, apparently full of sap, 40 thin shavings were taken, six inches in length, and half an inch broad. The wood from which these shavings were planed, weighed 12·37 grammes, and the shavings, when thoroughly dried in the stove, weighed 7·5 grammes*.

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* As the heat excited by the plane in taking off these shavings, was sufficient to evaporate a very sensible quantity of sap belonging to the wood from which they were cut, the shavings became perceptibly dry during the operation; for I found that 40 thin shavings sometimes lost more than one gramme (about 1-12th of their weight) in less than a minute. In order to obtain their true weight, whilst they still remained part of the wood, I adopted the precaution of weighing the piece of wood, both the moment before, and the moment after the operation of planing. The difference in the weight of the wood, under these two circumstances, indicates the weight necessary to be given to the shavings, and which is here always attributed to them.

We

Sap, &c. in different kin of wood.

We hence conclude, that a cubic inch of this wood, in its original state, while the tree was still alive, contained 7·1531 grammes of ligneous parts, which formed the fabric of the wood, and 4·3369 grammes of sap, differing in its specific gravity, little or nothing from common water.

As one cubic inch of the solid parts of this kind of wood weighs 29·45 grammes*, the 7·1531 grammes of ligneous parts found in a cubic inch of the trunk of the living tree, in September, could only have occupied the space of 0·24289 cubic inch.

And the 4·3369 grammes of sap, contained in it, only........................ 0·21880

Consequently, in one cubic inch of this wood, there was a bulk of air equal to.. 0·53831

Total............ 1·00000 cubic inch.

The difference between the structure of the oak and of the poplar, becomes very conspicuous on making a comparison, according to the subjoined method, between the constituent parts of these two kinds of wood, both in a growing state.

Thus, a cubic inch of wood is composed of,

	Ligneous parts.	Sap.	Air.
The oak	0·39353	0·36122	0·24525
The poplar	0·24289	0·21880	0·53831

This striking difference, in the proportions of the ligneous substance of sap and of air, discovered in these two species, sufficiently explain the difference observable in their weight and hardness. This inquiry may probably lead to other discoveries of more general utility in the study of the vegetable economy.

SECTION III.

Of the relative quantities of Sap and Air found in the same Tree, in Winter and in Summer; and in different portions of the same Tree, at the same time.

The following experiments were undertaken with a view to

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* See the table, page 323.

discover

Sap, &c. in wood at different seasons.

discover the difference between the quantities of sap and air found in the wood composing the trunk of a large tree, in winter and in summer.

On the 20th of January, 1812, I had a lime-tree felled, of about 25 or 30 years-growth, which had stood among several others of the same age in my garden at Anteuil. On taking a piece of wood from the middle of the trunk, at about three feet above the ground, it appeared to be filled, and even drowned in sap. Its specific gravity was 76017; consequently, one cubic inch of the wood weighed 15 788 grammes.

Having planed off 10 grammes of thin shavings from this piece, and dried them thoroughly in the stove, I found their weight reduced to 4·72 grammes.

Thus in possession of the specific gravity of the solid part of this wood, it was easy to determine, with the aid of these data, the constituent parts of a cubic inch, which were as follow:

Ligneous parts	0·25353 cubic inch.
Sap	0·44549
Air	0 30098
	1·00000

On the 8th of September, in the same year, (1812) I had a piece of wood (=5·84 cubic inches) cut from the trunk of another lime, of equal age with the former, (from 25 to 30 years) at the height of three feet above the earth. This tree was in a growing state, and the piece taken from it, after it had been trimmed by the joiner, weighed 87·8 grammes, and displaced 115·8 grammes of water, at the temperature of 62° F. consequently, its specific gravity was 75820. In the month of January, the specific gravity of this same species of wood had been found to be 79617.

From the piece of wood taken from the tree on the 8th of September, I had 14·19 grammes of their shavings planed off, which, after they had been thoroughly dried in the stove, weighed only 7 35 grammes. Hence we have, as the constituent parts of a cubic inch of this wood—

Sap, &c. in wood.

Ligneous parts	0·26489 cubic inch.
Sap	0·36546
Air	0·36965
	1·00000

From the results of these two experiments, we may conclude, that the body of a tree contains more sap in the winter than in summer, and more air in summer than in winter. But the following experiments demonstrate the sap to be very disproportionately distributed in the several parts of the same tree, at the same season.

On the 8th of September, I had a branch, about three inches in diameter, cut from the lime just spoken of, and which issued from the trunk at the height of ten feet above the surface of the earth. From the lower end of this branch, I took a piece of wood, and subjected it to the investigation requisite to ascertain its constituent parts.

Its specific gravity was 70201. The same day, I found the specific gravity of a piece of the trunk of the same tree, to be 75820.

Surprising as this difference appeared, my astonishment was still more excited, on finding that a piece of wood, of three years growth, cut from the upper end of the same branch, where it was but one inch in diameter, had a specific gravity of 85240.

There was, therefore, much more sap, and less air, in the wood of the upper extremity of the branch, than in the lower, which was nearer to the body of the tree.

I afterwards examined the young shoots of the current year, in the same tree, as well as in several other species of wood, and uniformly found that the specific gravity of the young wood, that is to say, of the current year, is always considerably greater than that of the same species of wood when grown older. Doubtlessly, because it contains more sap, and less air, than the old wood.

In the management of experiments for determining the specific gravity of wood of the current year, it is indispensably necessary to take an account of the space occupied by the pith, without which precaution, we shall be led to false conclusions.

Sap and volatile parts in wood.

I found the specific gravity of the oak of the current year to be 116530; that of the elm, 110540. Young shoots of these trees, deprived of their bark and pith, descend rapidly on being thrown into water; whilst pieces of the same tree, more advanced in age, swim on the surface, even when the wood is green, and more full of sap.

This fact is worthy the attention of persons occupied in the study of vegetable physiology.

I was next curious to examine the root of the lime from which I had already had one piece of wood from the trunk, and two pieces from one of its branches. With this view, on the 8th of September, 1812, I caused one of its roots, of about two inches diameter, to be taken up, and cut from it a piece weighing 93·25 grammes, which displaced 115·8 grammes of water. Its specific gravity was 80527, and, consequently, greater than that of the wood extracted from the trunk of the same tree, but less than that cut from the upper end of one of its branches. 20 48 grammes of thin shavings, from this piece of the root of the lime, weighed only 10·85 grammes after being thoroughly dried in the stove.

From these data, I determined the constituent parts of a cubic inch of the root, thus:

Ligneous parts	0·28775 cubic inch.
Sap	0·37358
Air	0 33867
	1·00000

The constituent parts of a cubic inch of the body of the same tree, were, as we have shewn:

Ligneous parts	0·26489 cubic inch.
Sap	0.36546
Air	0·36965
	1·00000

The constituent parts of a cubic inch of the wood of the same tree, taken the same day from the lower extremity of a branch, were:

Ligneous

Sap and volatile parts in wood.

Ligneous parts	0·25713 cubic inch.
Sap	0·27513
Air	0·46774
	1·00000

Lastly, the constituent parts of a cubic inch of the wood, taken near the upper extremity of the same branch, were:

Ligneous parts	0·25388 cubic inch.
Sap	0·47599
Air	0·27013
	1·00000[*]

For the more easy comparison, of the results of these four experiments upon the wood of the lime tree, made on the same day, with different portions of the same tree, I have collected them together in the following

TABLE.

	A cubic inch of wood was composed of		
	Ligneous parts.	Sap.	Air.
The root	0·28775	0·37358	0·33867
The trunk	0·26489	0·36546	0·36956
The lower end of a branch	0·25713	0·27513	0·46774
The upper end of ditto	0·25388	0·47599	0·27013
Wood taken from the trunk of a lime tree of the same age, on the 20th of Jan.	0·25353	0·44549	0·30098

Being desirous to ascertain whether a difference considerable enough to be valued, existed between the wood of the heart, or core, and the sap wood found between the rhind and the body of the same tree; I took on the 11th of September, an elm faggot, five inches in diameter, lopped from a large tree, which had been felled on the 20th of the preceding April, and had two cylindrical pieces, each six inches in length, cut out of it. The thickest of these taken from the core, weighed 191·05 grammes, and displaced 194·45 grammes of water; the other,

Sap and volatile parts in wood.

consisting of the sap-wood, weighed 93·61 grammes, and displaced 111·45 grammes of water.

The specific gravity of the core was, therefore, 98251; that of the sap-wood, 81764. But as the faggot had lain exposed to all the summer rains, the wood was far from being dry. I was, however, much surprised at discovering, that the core of this wood was more charged with sap, or water, than that of the same kind of wood when in a growing state. A fact which induces a suspicion, that the sap in trees is not enclosed in vessels or tubes apparently impervious to that liquid.

To obtain a better knowledge of the wood in question, I planed off 40 shavings, six inches in length, and half an inch in breadth, from a small board cut from the core; with an equal number of shavings, of similar dimensions, from another board cut from the sap-wood.

The 40 shavings from the core, taken just as they were planed off, weighed 16·37 grammes, and 10·53 grammes after they had been thoroughly dried in the stove.

The 40 shavings of sap-wood weighed 16·97 grammes before they were dried, and 11·99 grammes afterwards.

Thus possessed of the specific gravity of the solid parts of this kind of wood, it only remained to determine, from these data, the constituent parts of an inch of the wood, which was readily performed, as follows:

	Ligneous parts.	Sap.	Air.
In the core of the elm. .	0·41622	0·35055	0·23223
In the sap-wood.........	0·38934	0·23994	0·37072

It appears, from the results of these experiments, that the sap-wood of the elm contains rather more ligneous parts in its timber, than the core of the same tree; and that it contains much less sap, and more air. But as the tree had been felled nearly five months before it became the subject of investigation, it is very possible that the sap wood had become much drier than the core of the tree.

I had purposed to repeat these experiments upon wood in a growing state, and upon seer-wood; but the interference of other occupations has prevented a continuance of the inquiry. It cannot, however, but lead to results curious in themselves; and I therefore recommend it to the notice of all students in vegetable economy, as well as to those who love that noble science, and feel a gratification in being able to remove the veil under which the mysterious operations of nature are concealed.

Sap and volatile parts in wood.

The particular object which I had in view in exploring the structure of wood, have led me by a way by no means likely to be fertile in interesting discoveries but I have begun the work, and feel myself bound to complete it, in preference to every other consideration. These fascinating researches, I am aware, have already carried me too far, and I must now resign them into the hands of others, in order to fulfil my engagements. This I do most cheerfully, and it will give me the greatest pleasure to behold a field too long neglected once more broken up.

SECTION IV.

Of the Quantities of Water contained in Woods considered as dry, or Seer-Woods.

Wood is an hygrometric substance, and when exposed to the atmospheric air, always imbibes a visible quantity of water; varying, however, with the temperature and humidity of the air.

If the moisture in the wood were confined in vessels so constructed as to be totally impervious to water, the fabric of the wood would be uniformly the same, with the exception only of the variations caused in its dimensions by change of temperature; in which case it would be very easy to determine the quantity of water contained in the wood, when the specific gravity of its solid parts was known. But as the bulk of all woods is considerably diminished in drying, the experiment is rendered rather prolix, though by no means difficult, and its results are clear and satisfactory.

A few examples will suffice to point out the method to be pursued.

The composition of the oak, in a growing state, at the beginning of September has been already given. In order to ascertain the change which this wood undergoes by the process of drying, I made the following experiment.

From a faggot of oak, 5½ inches in diameter, which, covered with its bark, had been exposed to dry in the open air, for 18 months, I took a piece of rather more than an inch square, and six inches in length; it was good fire-wood, and seemed very dry.

This

Sap and volatile parts in dry wood.

This piece, after being trimmed by the joiner, weighed 126·2 grammes, and displaced 157·05 grammes of water; its specific gravity was consequently 80357, and a cubic inch weighed 15·939 grammes.

Forty-three shavings of this wood, six inches long, and half an inch broad, weighed 17·9 grammes; but when thoroughly dried in the stove, they were reduced to 13·7 grammes. They were, therefore, prior to being put into the stove, composed of 13·7 grammes of solid parts, that is to say, of dry, or seer-wood, and 4·2 grammes of water.

The results of this experiment indicate, that 100 kilogrammes of this excellent fire-wood contained 76 kilogrammes of seer-wood, and 24 of water; which is, probably, the ordinary state of the best fire-wood sold in the timber-yards of Paris, and all other places.

Were the wood to be kept for several years, in a dry place, secured from the rain, it is possible, that it might become dry to such a degree as to contain only about 12 per cent. of water, and 88 of seer-wood. But it will appear in the sequel, that wood of any kind, exposed to the atmosphere, could never become more dry, on account of its hygrometric quality, which it constantly preserves.

The following are the constituent parts of a cubic inch of fire-wood employed in this experiment:

Ligneous parts, or seer-wood.	0·40166 cubic inch.
Sap, or water.	0·18982
Air.	0·40852
	1·00000

Thus we are enabled clearly to demonstrate the difference between the oak in a growing state, and the same kind of wood after it has been felled and dried in the air, secured from the rain, for 18 months.

	Dry wood	Water.	Air.
In a cubic inch of oak, in a growing state.	0·39353	0·36122	0.24525
In a cubic inch of the same kind of wood, after it had been felled and dried for 18 months.	0·40166	0·18982	0·40852

By comparing the relative quantities of sear-wood contained in a piece of timber while in a growing state, and in the same timber after it has been dried, we may ascertain how much its fabric has shrunk by dessication.

It appears from these experiments, that the oak sold in the timber-yards of Paris, for fire-wood, contains rather more than one-half of the sap which it formerly had, in a growing state.

I have made several similar experiments upon other species of wood; but their results are better calculated for exhibition in a table, than for circumstantial detail.

(To be continued.)

III.

Description of the perpendicular Lift erected as a Substitute for Locks on the Worcester and Birmingham Canal at Tardebig, near Bromsgrove. By Mr. WOODHOUSE. *From a printed Letter of Mr. Edward Smith, of Birmingham, and the Reports of W. Jessop, Esq.*

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

THE whole of the machinery is under cover; and we entered the building at the lower level of the canal, where the appearance of a number of large wheels, rods, and chains, seen in perspective, had a very striking and pleasing effect. We walked by the side of an oblong trough or vessel, filled with water, large enough for a canal boat to float in. This reservoir of water, with the canal boat, weighs sixty-four tons, and is suspended by eight rods and chains over as many large cast-iron wheels or pullies, which are balanced on the other side of the wall by an equal number of square frames, loaded with brick-work, or other heavy materials. After examining the lower structure of the building and machine, we got into an empty boat which floated in the reservoir, and were slowly raised to the upper level of the canal, without any noise or jarring of the machinery, by means of wheels and pinions on the other side, which were worked by two men with great ease; it took about three minutes to ascend twelve feet, the difference between the two levels. When the trough is thus raised to the necessary height, the paddles at the end, which are ingeniously con-

Machinery for raising and lowering boats upon canals, without the same expence of water as by locks.

contrived for the purpose, being drawn up, a communication is made between the water in the trough and that in the canal, and the boat passes from the trough into the upper level of the canal, to pursue its course. In case a boat be ready in the upper level, it is, in turn, floated into the trough; the communication is then stopped by letting down the paddles into their places, and the machine is made to descend by the same means to the lower level of the canal, where, by similar paddles, the boat is released to proceed on its journey. Whether the boat be loaded or empty, it makes no difference in the weight; for, as the machine is kept filled to a certain height with water, the boat, on its entrance, displaces just as much of this fluid as is equal to its own weight.

I had previously formed a very erroneous idea of the machine, and fancied it was complex and might be easily injured, and thus rendered useless; but, so far from being complex, nothing can be more simple; the wheels, rods, and chains are strong enough to bear a far greater weight; and if one-half of the number were removed, or could be supposed, by any accident or design, to be out of order, the remainder would do the work; and if the pinions, &c. should, by any means, be deranged, the machine, with little trouble, would act without them, the reservoir being balanced by the weights suspended.

The great desideratum upon this canal is to procure water sufficient to answer the purpose of navigating down to the Severn. In case the six feet locks are adopted, the water must be raised, by steam engines, from the Severn, and thrown back for sixteen miles, to the summit at Tardebig. Thus there must be an immense expence incurred in the construction of such a number of engines as would be requisite for this purpose, and also in the consequent charges for the supply of fuel, repairs, and the regular working the engine. This may easily be calculated from the allowed data. But, in case the LIFTS should be adopted throughout, there will be very little waste of water, perhaps not so much as is constantly forcing its way through, or under a canal lock gate, when the lock is worn, or shaken by accident or mismanagement.

The expence of erecting these perpendicular lifts must be, however, necessarily, very great, besides the constant expence of two or more men stationed at each, to work it. The present

sent lift is only twelve feet, and by way of experiment: for the Committee, in the first instance, did not choose to run too great risk; but the machines may be adapted to raise twenty, thirty, or any number of feet by greater length of chains, and adequate building to suit the levels, at a much less proportionate expence than in shorter lifts. To what extent this may be carried, prudence and experience must dictate; and, therefore, whether the expence of the perpendicular lifts, or the old system of lockage, with the expences of procuring water, be greater, it would be improper for me to give an opinion.

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

In the course of conversation, many circumstances, highly favourable to his plan, were mentioned by the Inventor, which appeared to me to have great weight. By the old plan, each of the locks must have the same fall, and in each range they must be built near to each other, so as to be under the eye of the lock-keeper; of course, instead of adapting them to the nature of the country, a great expence must unavoidably take place in the forming the land to the lock. This will not be the case with the lifts; being quite distinct from each other, it will not signify whether they be close, or one or six miles asunder, nor whether they lift 12, 20, or 30 feet. They may be accommodated to the nature of the country through which the canal passes, will require much less land, and may be placed, probably, in situations where the land is of least value: the canal, for the same reason, may vary its course according to circumstances, which cannot be the case in the old system; and when we consider the great price of land in some situations more than others, the saving to a Canal Company, in this respect, must be very great.

As soon as one lift is finished in a canal, it may be used, and a great saving made in water carriage to the remaining works, and perhaps the tonnage constantly increasing, which is not the case in the lock system, which cannot be used to much effect till the steam engines are completed, and the water brought to the highest level. And if any considerations should be thought to stand against their general adoption in all circumstances, still the lifts would be of value; wherever water was scarce, and the lock system might be followed in situations where it was in plenty.

At the time I am writing, I have before my eyes, 150 yards from

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

from my house at Bordesley, a large fire engine, which till of late was an insufferable nuisance to the neighbourhood, by the immense volumes of thick black smoke it was pouring out, night and day, without intermission. This inconvenience of the smoke has been lessened, in a great degree, by a contrivance in the management of the fire place, which ought to be adopted in all such cases. When I observe this engine employed solely in throwing back water to the upper level of the Warwick canal, for the floating of the boats, up and down through half a dozen locks, within the space of half a mile, I cannot help considering, that had the lift been known and applied, the canal might, at a little expence, have been continued on a level to the place where the fire engine is constructed, the expence of working the engine and all the lockage saved, and the boat, by one lift removed from one level to the other; the first cost of the lift, no doubt, would be great, and then you have said nearly the whole; no fear of dry seasons, the reservoirs and feeders being sufficient to supply the loss of water from exhalation by the summer sun.

Again, to look at the Birmingham canal at Smethwick, with its fire engine, reservoirs, and double range of locks---to what advantage might this machine be applied in such a situation!

Plate VIII. Is a perspective view of the internal part of the machine, when viewed from the lower level of the canal. The surrounding walls are taken away to avoid confusion: the centre wall is also broken off, at the nearest end, in order to shew the manner in which the balancing weights, at the back, are suspended. The better to display the construction of the trough, it is raised five feet above the lower level; the dimensions of it are as follows; length 72 feet---breadth 8 feet---depth 4 feet 6 inches, all outside measures. It is composed of planks, 3 inches thick; its weight, when filled to the proper height with water, is 64 tons. The paddles, and their appurtenances, are marked as clearly as the nature of the case would admit; a further explanation of them, will be found in the references to figures 2, 3 and 4, Plate IX.

From the corners of the trough, rise four strong posts, 12 feet high, in each of which is a groove, which receives the respective paddles. Parallel to these, are similar posts, in which slide the paddles of the canal. When a boat is to be introduced at the lower

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

lower level, the narrow space between the paddle of the trough and that of the canal, is first filled with water, by opening a valve, the situation of which is pointed out by the letter *H*, in fig. 2 and 4, Plate IX.; the lateral pressure of the water against the paddles, is thus removed: The small chain, which hangs down between the upright posts, the lower part of which is double, is then linked to the hooks of both paddles, and by means of the crane near the end, they are drawn up together, and the boat floats into the trough; the paddles are then dropped, and the trough raised to the upper level, when the boat is liberated by opening the paddles at the contrary end. A similar operation takes place, when a boat is required to descend from the upper to the lower level.

Plate IX. Figure 1, a section of the end of the machine; this clearly shews the principle by which the weight is raised, viz. that of the simple pulley, where the weight suspended on each side being equal, a force sufficient to overcome the unavoidable friction being applied, puts the whole in motion either way. *A A* represents the section of the trough, suspended from the iron beam *C D C*, by rods, the lower ends of which are fastened by screws and nuts at *B B*, and the upper ends are fixed in the same manner at *C C*.

From the centre *D* of the beam *C D C*, proceeds a very strong double chain *D D d E*, passing over the wheel *H H*. From the end *E* hangs an iron rod *E F G*, which passes through a thick square platform of oak at *G*, loaded with brick-work to the weight of eight tons; this is the case with each of the others; the weight, therefore, of the whole, is 64 tons, being equal to that of the trough, which they hold in equipoise.

H H, a cast-iron wheel, 12 feet in diameter, one of the eight which are seen in Plate I. *I K*, the centre wall, 30 feet high.

From *L*, under the centre of the trough, is suspended a chain, which is loaded at equal distances with blocks of iron, 1, 2, 3, 4, 5; the weight of them is equal to as much of the chain and rod *D d E F G*, as hang in a perpendicular direction. The weights *F G*, are provided with similar chains, so that, as in the present instance, when they are at the lower level, the opposite blocks are called into action, and counterbalance the force of that portion of chain and rod, extending from *d* to *G*, while the

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

blocks suspended from *G* lie inactive in the cavity *M N*: the contrary is the case when the trough is sunk.

Figure 2, is a section of the paddles, &c. of the upper level. *Aa*, *Bb*, the two perpendicular posts, containing the grooves, in which the paddles *C D* slide. *E F*, small wheels at the extremities of the posts; these, by rolling against other surfaces, contribute to regulate the ascending and descending motion of the trough. This is more distinctly seen in Figure 3, which is a profile of this part.

G G, is the bottom of the canal, which projects a little beyond its paddle, in order to fill up the space between the bottoms of the two paddles, through which the water would otherwise escape. The side spaces are filled up by square pieces of wood, slide against strips of thick felt; thus rendering the whole completely water-tight. *H*, the small valve, by withdrawing which the space between the paddles is filled with water.

Figure 4. A plan of the situations of the paddles in the grooves; which is sufficiently explained by comparative reference to the other plates.

Plate X. gives an elevation of the back of the machine, shewing the eight wheels *H* 1, *H* 2, &c. the chains and rods *D E F*, and the poise weights *F G*. Here also the external building is removed, the centre wall alone remaining, in the interstices of which the wheels revolve.

The wheels No. 2 and 7. are toothed through twelve feet of their circumference, and by means of these teeth, they are acted upon by the wheel-work, which this plate also exhibits. *I I* are the two winches by which the pinions and wheels *K K*, *L L*, *M M*, *N N*, *O O*, are turned, and sufficient power is thus acquired to move the wheels *H H*; this is effected by the teeth of the pinions *O O*, meeting those of *H* 2 and *H* 7; these two being connected by the common axis *P P*, their motions necessarily correspond. On the sides of the weights *F G*, are small projections, which slide into grooves, constructed in the upright posts *Q R*. By means of these grooves, and the regulating wheels *E F*, in figures 2 and 3, Plate II. the perpendicular motion is rendered so perfectly true, that it was judged unnecessary to give the wheels *H H*, any hollow; the chains consequently move on flat surfaces, depending only on the mathematical truth of the work.

Machinery for raising and lowering boats upon canals without the same expence of water as by locks.

The lines *S*, *T*, point out the situations of the lower and upper levels of the canal, between which, as was before observed, the fall is 12 feet.

W. H. SMITH.

Since my letter was first written a number of improvements have been made in the perpendicular lift by the ingenious projector, to prevent the chance of accidents to which new schemes are exposed, and also to obviate several objections that have been industriously circulated against the machine. An apparatus has been added, which effectually prevents the sudden motion of the machine, in case any accident should happen while the conductor is at the lower level, and which renders it impossible for the weights to descend, till the paddles of the conductor are adjusted.

Two pumps have also been introduced, which regulate the speed of the machine, and by which the conductor may pass from one level to the other, without any manual labour; and the conductor and paddles are now so guarded, that they cannot receive injury from the violent entrance of the boats.

Owing to the numerous delays, the tunnel at Tardebig was not completed so soon as stated, and the consequent trial of the lift, as expected by the proprietors, could not be effectually made previous to the general meeting of January 1, 1811. This, and other circumstances, induced the general meeting to pass resolutions, by which it was determined, (though so much expence had been incurred) to abandon the scheme *in toto*, and finish the canal by means of locks; chiefly, however, on the ground, that it was impossible (as alleged) for the lift to pass nearly the number of boats requisite, when the canal should be completed. Several respectable proprietors, not satisfied with this determination, and concerned that a plan, in their opinion, replete with advantage to the canal and the public at large, should be abandoned almost without trial, have come forward at their own expence, to make a complete trial of the machine, and its capability to do what might be requisite.

This trial was continued under the patronage of these gentlemen for nearly a month, by means of three boats constantly working upward and downward, for a given period in each day, one of 20 tons, one of 15 tons, and the other empty, being the

usual

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usual proportion in the common traffic on canals. The result of this will surprize many persons who had formed a very different idea of the power of the lift, and must be very gratifying to the inventor, who, whether the scheme be adopted or abandoned by the proprietors, will be able to refer to the solid test of experiments thus laid before the public. The result of the experiments to the day of publication I subjoin

E. SMITH.

March 18, 1811.

Feb. 25,	1st Day,	50	Boats passed,	in 6 Hours 29 Min.
—— 26,	2nd	60	ditto	in 8 Hours 10 Min.
—— 27,	3d	70	ditto	in 9 Hours 8 Min.
—— 28,	4th	37	ditto	in 5 Hours 1 Min.
Mar. 1,	5th	50	ditto	in 6 Hours 40 Min.
—— 2,	6th	50	ditto	in 6 Hours 48 Min.
—— 4,	7th	50	ditto	in 6 Hours 52 Min.
—— 5,	8th	40	ditto	in 5 Hours 16 Min.
—— 6,	9th	51	ditto	in 6 Hours 21 Min.
—— 7,	10th	48	ditto	in 6 Hours 22 Min.
—— 9,	11th	41	ditto	in 5 Hours 20 Min.
—— 11,	12th	40	ditto	in 5 Hours 25 Min.
—— 12,	13th	100	ditto	in 11 Hours 46 Min.
—— 13,	14th	58	ditto	in 7 Hours 20 Min.
—— 14,	15th	50	ditto	in 5 Hours 41 Min.
—— 15,	16th	110	ditto	in 12 Hours.
—— 16,	17th	50	ditto	in 5 Hours 37 Min.
—— 18,	18th	50	ditto	in 5 Hours 54 Min.
—— 19,	19th	113	ditto	in 12 Hours.
—— 20,	20th	50	ditto	in 5 Hours 42 Min.
—— 21,	21st	68	ditto	in 8 Hours.
—— 22,	22d	60	ditto	in 6 Hours 26 Min.
—— 23,	23d	62	ditto	in 6 Hours.

(Signed) WILLIAM JOHNSON.

IV.

Curious Fact of the Outlines of Trees, accurately sketched on the surface of the ice on the Bog Lakes of Ireland. In a Letter from JOHN CHICHESTER, *M. D. of Bath.*

To W. Nicholson, Esq.

SIR,

Trees buried under the Bog Lakes are marked by the hoar frost being less perceptible over them.

THE account given in your Journal for April last, of the remarkable appearance of the ice in a pond in which a man lay drowned, brought to my recollection the following analogous, and perhaps no less curious phenomenon, occurring in the Bog Lakes of Ireland, communicated to me some years since by the Rev. Mr. Mangin. The following are Mr. Mangin's own words:—

"On the 24th of December, 1809, I was in company with a gentleman from Ireland, who mentioned what appeared singular, and was then new to me: speaking of the bogs in his neighbourhood, and of the large trees so frequently found in them, he said, that at those periods of the year, when the hoar frost fixes on the surfaces of the small lakes with which those morasses abound, he had repeatedly observed the form of a tree, (lying, perhaps, at a depth of fourteen or twenty feet beneath,) sketched most accurately on the ice above; that is to say, its length, breadth, and ramifications denoted by the frost not settling with equal force on those portions of the fluid under which the tree was extended, while the congealment was every where else more dense and complete." The gentleman added, that it was well known to the country people, who were accustomed to search for and find timber when thus indicated. The trees discovered in these places are of various kinds, oaks, elms, &c. and very commonly yew trees of vast size, their position invariably horizontal."

Without any comment, I beg leave to subscribe myself

Sir,

Your obedient Servant,

JOHN CHICHESTER, M. D.

And Physician at Bath.

May 13*th*, 1813.

Anno-

Annotation.

Though the indistinct outline of a large object not deeply immersed in a small stagnant pool, may seem to be well explained by the observations given at page 304, of our XXXIVth vol. Yet the same principles do not appear adequate to shew why the ramifications of a tree buried twenty feet in a bog, should be neatly figured upon the ice of water lying on its surface. None of the general operative powers with which we are acquainted, present a solution of this effect. Heat, electricity, gravitation. Of these the latter only is known to act in the perpendicular; but this affords no ground for the remotest conjecture. It would be desirable to know whether the ice of the outline were different in texture from the rest.—W. N.

V.

On Copper Wire, guilt with Brass. In a Letter from a Correspondent.

To Mr. Nicholson.

SIR,

Guilding by means of brass.

I HAVE been informed, that the gilding of copper wire by means of brass is carried to great perfection in Germany. Perhaps it may also be the case in England; respecting which I should be glad to hear from your correspondents. The facts, as stated to me, are, that copper wire, coated with brass, is capable of being drawn out to the fineness of a hair; much finer than copper alone; that it is used for making gold lace, and the process effected in the humid way, as follows:—Take of zinc one part, and of mercury twelve parts—make a smooth soft amalgam, to which, if a little gold be added, it will be better. Clean the copper very nicely with nitric acid; put the amalgam into muriatic acid, and add argol or crude (not purified) tartar. Boil the clean copper in this, and it will be very finely gilt. Epaulets and gold-coloured trinkets are thus made very beautiful.

Query. Would this be an improvement in pins?

I am, Sir,
Your Constant Reader,
M. M. B.

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ERRATUM.

Page 152, line 30, *for* "repertory," *read* "retrospect."

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